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Art. I.—*The Smithsonian Institution.*

We have been silent but not uninterested spectators of the controversy which has been so rife for a year past respecting the Smithsonian Institution. Now, however, when the discussion appears to have drawn to a close, or at least when the arguments on both sides are well nigh exhausted, it seems proper, in view of the relations which this Journal sustains to science and to the public, that we should take this important subject into dispassionate consideration, and place our conclusions upon record.

The official documents with which we have to do, are:

1. The will of the founder, James Smithson.
2. The Act of Congress entitled "An Act to establish the Smithsonian Institution for the increase and diffusion of knowledge among men," passed in August, 1846.
3. The eight annual Reports of the Board of Regents of the Institution, made to Congress, from 1847 to 1854.

All these are included in the Eighth Annual Report of the Board, showing the operations, expenditures, and conditions of the Institution up to Jan. 1, 1854, and the proceedings of the Board up to July 8th, 1854.

And further, as to the points in controversy:

4. Report of the special committee of the Board of Regents, Hon. Mr. Pearce, Chairman, relative to the distribution of the income of the Smithsonian fund, made May 20, 1854, published in the Proceedings of the Board, forming a part of the eighth annual Report, above mentioned.

5. The Hon. Mr. Meacham's minority Report from the above committee, made subsequent to the adjournment of the Board of Regents and the presentation of their annual Report to Congress, but appended to the House of Representatives' edition of said Eighth Annual Report.

6. The Proceedings of the Board of Regents at the annual meeting in January, 1855;—an abstract from the records, published in the National Intelligencer of Jan. 17th, 1855.

7. A letter of the Hon. Rufus Choate to the President of the Senate and the Speaker of the House of Representatives, resigning his office of Regent of the Smithsonian Institution; and the Proceedings in the Senate consequent thereon. (Congressional Globe, &c.)

8. Report of the Judiciary Committee of the Senate, to whom was referred the inquiry whether any, and if any, what, action of the Senate is necessary and proper in regard to the Smithsonian Institution; made Feb. 6, 1855.

9. Speech of the Hon. W. H. English in the House of Representatives, Feb. 27, 1855, on the Smithsonian Institution.

10. Report of Hon. Mr. Upham, Chairman of the Select Committee of the House of Representatives, to which was referred the letter of the Hon. Rufus Choate resigning his place as a Regent of the Smithsonian Institution, with instructions to enquire and report to the House whether the Smithsonian Institution has been managed and its funds expended in accordance with the law establishing the Institution, &c. &c. (Printed in the Boston Daily Advertiser, March 10; the official publication not yet issued.)

11. Hon. Messrs. Witte and Taylor's Report from the same committee. (Printed in the Boston Daily Atlas, March 16th.)

These documents all are, or readily may be, accessible to our readers in this country; and we are obliged to presuppose on their part a full acquaintance with them. However desirable a complete analysis of them might be, and however needful for the information of our foreign readers interested in a question of more than American concernment, our narrow limits forbid this, —forbid even that comprehensive historical view of the character and operations of the Institution which we would gladly give, and require us, after thus indicating the official documents which comprise the whole case, to enter at once upon the consideration of the essential points of this unhappy contest.

This controversy has grown out of certain widely divergent views which are entertained respecting the design and functions of the Institution. One party, that opposed to the policy actually adopted, contends that the Institution is by law, and should be made in fact, a *library*;—that from its income, now of about $31,000, an average amount of $25,000 should be annually expended for books. Other specified objects may subordinately or contingently come within the scope of the Institution; but the library, with its "legal possession of twenty-five thousand dollars a year" is "the paramount interest of the Institution," or would be, "had the Institution been made, as Congress intended, almost

entirely a library."* This is the view taken by Messrs. Choate and Meacham, of the Board of Regents. Another party, comprising a large majority of the present Board, maintains that a library is not the end or purpose, nor *an end* of the Institution, but is one of the instruments or means by which the objects of the establishment are to be subserved ;—these objects being "the increase and diffusion of knowledge among men," as expressed in the will of the founder, and affirmed in the act of Congress which established the Institution and prescribed the way in which they are to be attained. Of the large majority of the present managers of the Institution, coinciding in this general view, some would give the library more, others less prominence: but they all agree in principle as to their duties and obligations under the law,—maintaining as they do, in opposition to the first-named party, that they are not required to vote an annual appropriation of about $25,000 for the library, nor of anything like that amount, except when in their judgment it may be most expedient. In fact, to use the language of one of the number, they are "opposed to making the library *the principal or controlling* feature of the Institution," but "regard it as *one* of the important instruments to be used in accomplishing the desired end."†

This latter view has constantly prevailed in the Board of Regents charged with the management of the Institution,—very largely prevailed, we may add; for most of those who have favored the amplest appropriations ever proposed for the library, have evidently done so in the exercise of a discretion over the amount, which they understood to be conferred upon them by the law. By good majorities the Board of Regents have directed the action and operations of the Institution so as not to make the library "the paramount interest" or controlling feature of it, but only one among several means or instruments of increasing and diffusing knowledge. Hence the controversy, which has been carried on with so much spirit and feeling, in the Institution and elsewhere.

The voluminous minority Report of Mr. Meacham (Doc. No. 5, supra), and the able and eloquent arguments of Mr. Choate having failed to induce the Board of Regents to change their course, an appeal, in effect, was made to Congress, in a letter of resignation of his office of Regent by Mr. Choate ;—the result of which is to be found in the discussion which arose thereupon in the Senate (Doc. No. 7); in the Report of the Judiciary Committee of that body (Doc. No. 8)—chosen from among its ablest jurists,—unanimously approving the action of the Board of Regents; and in the two reports from the select investigating com-

* Report of Mr. Meacham; document No. 5, in the enumeration given above.

† Hon. Mr. English. Speech in the House of Representatives; document No. 9, supra.

mittee raised in the House of Representatives; that of its Chairman, Mr. Upham (Doc. No. 10), disapproving, that of Messrs. Witte and Taylor (Doc. No. 11), fully approving the course pursued by the Regents.

The questions at issue, it will be seen, are two, which should not be confounded, as they generally have been, at least in the popular discussions of the subject. The charges brought against the Board of Regents and their Secretary, when examined, are found to be: 1st, that they have *improperly exercised the powers conferred upon them:*—a charge which may involve nothing more than a difference of opinion as to the best means for attaining the desired ends; or it may impeach their judgment, or their motives. 2d, They are also charged with having *disregarded and violated the law* from which their powers and their existence are derived. This much graver charge involves a legal question,—the one upon which the gist of the controversy turns. If the questioned action of the Board is illegal, it matters little whether it would otherwise be expedient and judicious,—whether it would or would not tend most effectually to further the object in view. If, on the other hand, it should prove to be warranted by the law, we should have only its wisdom and propriety to consider.

We have heard the charges: let us now look to the law.

The purport of the law, as its preamble declares,* is to accept the trust created and proffered by the testator, and to found an establishment for "the faithful execution of said trust according to the will of the liberal and enlightened donor."

The whole of Smithson's will which relates to this bequest is comprised in the words; "I then bequeath the whole of my property, subject to the annuity of £100 to John Fitall, and for the security and payment of which I mean stock to remain in this country, to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men."

The "act to establish the Smithsonian Institution," consists of the preamble cited in the note below, and of eleven sections.

The first constitutes the *Establishment*,—a corporation in fact, with perpetual succession, but without the corporate power of suing and being sued,—to consist of ten specified high officers of the United States *ex-officio*, of the Mayor of Washington, and of such persons as they may elect honorary members.

* "James Smithson, Esq., in the kingdom of Great Britain, having by his last will and testament given the whole of his property to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an establishment for the Increase and Diffusion of knowledge among men; and the United States having by an act of Congress received said property, and accepted said trust, therefore, for the faithful execution of said trust according to the will of the liberal and enlightened donor, Be it enacted," &c.

The second section appropriates the accrued interest of the bequest, or so much of it as may be required, for the erection of suitable buildings, and for current incidental expenses; and funds the capital, for the perpetual maintenance of the Institution.

The third section creates a Board of Regents to conduct the business of the Institution, fixes the mode of appointment of the elective members of the Board (the *ex-officio* Regents being the Vice President and the Chief Justice of the United States, and the Mayor of Washington), as well as their term of service, authorises the Board to appoint a Secretary of the Institution, who is likewise to be the Secretary of the Board of Regents, prescribes the manner of transacting business, and requires an annual report to Congress.

The fourth section directs a building to be erected.

The fifth relates to the site and character of the building; and requires it to have "suitable rooms or halls for the reception and arrangement, upon a liberal scale, of objects of natural history, including a mineralogical and geological cabinet; also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms.

The sixth section bestows upon the Institution objects of art and natural history belonging to the government.

The seventh section enacts; "that the Secretary of the Board of Regents shall take charge of the building and property of said Institution, and shall, under their direction make a fair and accurate record of all their proceedings, to be preserved in said Institution; and the said Secretary shall also discharge the duties of librarian and keeper of the museum, and may, with the consent of the Board of Regents, employ assistants; and the said officers shall receive for their services such sums as may be allowed by the Board of Regents, to be paid semi-annually on the first days of January and July; and the said officers shall be removable by the Board of Regents whenever, in their judgement, the interests of the Institution require any of the said officers to be changed."

Section 8, enacts that the members and honorary members of the Institution (meaning thereby the establishment constituted by the first section) may hold stated and special meetings, "for the supervision of the affairs of said Institution, and the advice and instruction of said Board of Regents;" and continues:—

"And the said Regents shall make, from the interest of said fund, an appropriation not exceeding an average of $25,000 annually, for the gradual formation of a library composed of valuable works pertaining to all departments of human knowledge."

Section 9. "And be it further enacted; that of any other moneys which have accrued, or shall hereafter accrue, as interest upon the said Smithsonian fund, not herein appropriated, or not required for the purposes herein provided, the said managers are

hereby authorised to make such disposal as they shall deem best suited for the promotion of the purpose of the testator, anything herein contained to the contrary notwithstanding."

Section 10, requires those taking copyrights to deliver a copy of the publication to the Librarian of the Institution, and another to the Librarian of the Congress library.

Section 11, and last, reserves the right of altering, amending, adding to or repealing the provisions of the act.

The Smithsonian Institution, thus founded, is required by the fifth section *to have* a building suitable for its purposes, lecture-rooms, and, by necessary implication, a museum of natural history, including a geological and mineralogical cabinet, a chemical laboratory, a library, and a gallery of art. But what is the Institution *to do*? Does the mere custody of these instrumentalities accomplish the object of the trust? Or is it through their use that the object in view, viz, "the increase and diffusion of knowledge among men" is to be furthered. And if the latter, then by whom furthered? Will the mere access of the public to the books and cabinets (implied, we may well suppose, but nowhere even indirectly alluded to or provided for) increase and diffuse knowledge among men to the greatest degree, or even largely. Who is to use the lecture-rooms or the chemical laboratory and other means of research, by the mere inspection of which small increase of knowledge can be expected? Manifestly the law contemplates the fulfillment of the objects of the trust in the use of these collections, laboratories, and books, not alone by all comers promiscuously (under needful regulation), according to their various ability, but also, (and that not subordinately,) by the *Institution itself*, through action of its own, and agencies which it may set in operation;—modes of action nowhere prescribed in the law, indeed, being wisely left to the foresight and discretion of the Board, supervised by the Establishment, but for which an ample charter is conferred by the ninth section. The implication seems to be as direct in these respects, as that the ordering of lecture-rooms implies the delivery of lectures.

And how are the operations and the income of the Institution to be distributed among the several specified objects, and others deemed "best suited for the promotion of the purpose of the testator," which the managers are authorised to make appropriations for? All this, it appears to us, is plainly left to the wise discretion of the managers of the Institution, subject only to the limitation contained in the eighth section, fixing a certain but very high maximum limit to the sum which may be expended on the library.

The interpretation of this clause, however, is the turning point of the controversy. The Board of Regents have all along assumed this clause, directing an appropriation not exceeding an

average of $25,000 annually, for the gradual formation of a library, to limit their otherwise full discretion in one direction only,—to limit it upwards, not downwards. Their course of action from the first shows this. However diverse the views that have been entertained in the Board as to the propriety of large or small appropriations for the library, we are not aware that this full power and responsibility has been denied until recently. The report of a committee on the library (consisting of Messrs. Choate, Hawley, and Rush) appointed at the first session of the Board of Regents, declares, "that they see in the language of the act which the Regents are created to administer, and in the history of the passage of that act, a clear intimation that such a library was regarded by Congress as prominent among the more important means of increasing and diffusing knowledge among men. This intimation they think should control in a great degree the acts of the Regents." "And, without pausing to inquire what precise average amount should be expended, the committee will say that they have become satisfied that there would be no difficulty in judiciously expending, for a limited period, if it is otherwise desirable to do so, the entire sum indicated as the maximum in the act." And the resolution they reported for the appropriation of $20,000 for a library was at that time adopted. Thus, in advocating the largest views in respect to the library that have ever been presented by a committee or sanctioned by the Board,—at the time and place when those, if such there were, who believed the law to require the Institution to be made "almost entirely a library" would surely put forth the whole strength of their cause,—no claim is made for it as the "paramount object," no right whatever is asserted to the maximum sum mentioned in the act; but the large appropriation they sought to devote to the library "for a limited period," is recommended solely on grounds of expediency; although motives found in an intimation gathered from the history of the act are introduced as sanctioning the propriety of their recommendation. We seek in vain for any intimation that they were *bound* to propose so large a sum as they did, or that they would have done so had they believed a smaller appropriation more expedient at the time.

The next action of the Board of Regents, a month later, when the committee raised "to digest a plan to carry out the provisions of the act to establish the Smithsonian Institution," had made its report, brings us to the passage of the now famous "compromise resolutions," as they are called. In these it was agreed that, after the completion of the building, one half of the annual income, or $15,000, should be appropriated to the library, the museum, collections, &c.; and the other half to the preparation and publication of transactions, reports, and other publications of the Institution, to aiding and stimulating original researches in all

departments of knowledge, to the payment for lectures delivered at Washington, &c.; the Institution thus entering upon the career of active operations for the increase and diffusion of knowledge for which it is so distinguished. We know not by what majority these resolutions were adopted. Their legality, however, appears not to have been questioned. No member entered any protest upon the record, as he surely would have done if he held them to be an infringement of the law. That the minority raised no question of the illegality of this action may be clearly inferred from the terms of the resolution which they supported as a substitute. This resolution, for which we are indebted to Mr. Meacham's Dissenting Report (Doc. 20, 5), is as follows:

"*Resolved*, That it is the intention of the act of Congress establishing the Institution, and in accordance with the design of Mr. Smithson, as expressed in his will, that one of the principal modes of executing the act and the trust, is the accumulation of collections of specimens and objects of natural history and of elegant art, and the gradual formation of a library of valuable works pertaining to all departments of human knowledge, to the end that a copious storehouse of materials of science, literature and art, may be provided, which shall excite and diffuse the love of learning among men, and shall assist the original investigations and efforts of those who may devote themselves to the pursuit of any branch of knowledge."

It merely affirms that the library, the museum, and a gallery of elegant art constitute "one of the principal modes of executing the act and trust." It does not deny that the "active operations" may be *a legal*, or even a proper mode. That denial is more recent.

The experience of six years having rendered it doubtful whether this absolute division of the income into two equal parts, irrespective of circumstances as they occur, was practically the best mode that could be devised, in March, 1853, a special committee of seven was raised to consider this subject. This committee made its report on the 20th of May, 1854, by its chairman, Mr. Pearce, all the members concurring except Mr. Meacham, who had replaced Mr. Choate on the committee a few months previous. The report, (Doc. No. 4,) after a general view of the character, condition, and operations of the Institution, actual and prospective, submitted two resolves; the first, repealing the resolutions passed in January, 1847, and then about to come into effect, which required an equal division of the income between the active operations on the one hand and the museum and library on the other; the second requiring "That hereafter the annual appropriations shall be apportioned specifically among the different objects and operations of the Institution in such manner as may, in the judgment of the Regents, be necessary and proper for each, according to its intrinsic importance, and in compliance in

good faith with the law." A restriction which the Regents had imposed on themselves, antecedently to experience, is here proposed to be removed. As, in the exercise of their discretion, they had adopted a fixed and unvarying apportionment, they now proposed, instead, to exercise the same powers and the same discretion (neither more nor less) from year to year, according to their best judgment at the time, and in compliance in good faith with the law.

The consideration of these resolutions was postponed until the next annual session of the Board, in January of the present year; and it was ordered "that said report, and such report of a minority of the committee as may be made in the recess of the Board, be printed." The committee's report was so printed, for the use of the Board; and, belonging to the proceedings of the Board, it properly made a part of the Eighth Annual Report of the Regents to Congress, which was closed on the 22d of July, presented on the 1st of August; and ordered to be printed both by the Senate and the House of Representatives. Up to this time, an interval of two months having elapsed, no minority report had been sent in, so far as we can learn, and none could therefore properly be embodied in the Annual Report, "showing the proceedings of the Board up to July 8, 1854." A minority report of the Hon. J. Meacham, (Doc. No. 5,) without date, and consisting of an elaborate review and criticism of the report of the Special Committee, is however appended to the House edition of the Smithsonian Report,—whether properly or not we need not inquire.* In this document it is for the first time officially maintained (though it had been anonymously argued in the newspapers some time before, and apparently broached in the sessions of the Board as early as the spring of that year): 1, That the "compromise resolutions" of Jan. 16, 1847, *were unauthorized by the law; and yet ought to be continued in force:* and 2, That their *repeal by the passage of the resolves of the Special Commit-*

* Not that any one could wisely object that such an official document as this is, or would become when laid before the Board, should have the widest publicity. But, as it happens to be printed in the House edition of the Smithsonian Report, and not in that of the Senate, which was somewhat earlier published, it formed the occasion of a charge made by certain newspapers, that the Secretary of the Smithsonian Institution *suppressed* in the Senate edition Mr. Meacham's report adverse to his views. Whereas, we find, 1, That no minority Report *had been made*, and therefore none could form part of the annual communication to Congress: 2, That the manuscript was not sent in to the Secretary at any time during the recess, so that the Secretary could cause it to be printed for the use of the Board, as was ordered: 3, It does not appear to have been furnished to the Senate unofficially (officially it plainly could not have been), and so could not possibly have been suppressed there: 4, Having been unauthorizedly annexed to the Mss. Smithsonian Report for the past year communicated to the House of Representatives, while in the custody of its Committee on Printing, and the question being raised in this Committee, the Secretary, on being consulted, urged no objection to its being printed, with the simple statement of fact prefixed "that it does not form a part of the proceedings of the Board of Regents reported to Congress."

tee would be a further illegality, unless accompanied by a resolve, that a compliance in good faith with the letter and the spirit of the charter requires that a large proportion of the income should be appropriated to the library.

Let it be noted, in passing, that the argument of Mr. Meacham's report, and this resolve appended to it, do not present the very same issue,—unless we interpret the latter by the former, which, so far as respects the writer's meaning and intention, we may properly do. The proposition, "that a compliance in good faith with the letter and spirit of the charter of the Smithsonian Institute requires that a large proportion of the income of the Institution should be appropriated for a gradual formation of a library," is one thing; but is not identical with the proposition, so elaborately argued in the body of the report, that the enactment in the 8th section of the law, directing "an appropriation, not exceeding an average of $25,000 annually, for the gradual formation of a library, composed of valuable works pertaining to all departments of human knowledge," legally and morally enjoins the Board to expend this maximum sum, or something like it, on the library,—requires them to expend sums which in a term of years shall amount to an average of $25,000 *per annum*, whether convinced that this is the best use of the income, or not.

That an influential portion of the Board, especially during its earlier years, felt that the library was the safest, and ought to be the most prominent, object of their fostering care, there is no kind of doubt. Some of these, on further experience, have changed their minds. One of them is Mr. Pearce, the author of the Special Committee's report, who now so ably advocates the "plan of active operations;" although, as Mr. Meacham is careful to inform us, he strenuously supported "the library plan" in the discussion upon the bill before the Senate;—which clearly shows that he did not understand the law to prescribe 'the library plan,' but only to permit it. Another portion (of which Mr. Douglas may be taken as a representative), looking to the history as well as to the words of the act, hold that Congress "did sanction the policy of a library as a principal but not an exclusive feature in the Institution,"—"that the law contemplates the library as a prominent object in the Institution, and that at least a majority of the funds should be expended in the building up of the library;"—the intention of the legislators, even when not prescribed in restricting terms, being held to limit, or at least to influence to a great degree, the discretion of the administrators. Such persons would properly adopt the resolves appended to Mr. Meacham's report, while they would repudiate his argument.

The doctrine of that argument appears to be novel. Perhaps it pre-existed in a latent state, awaiting the heat of popular controversy to germinate it at a favorable moment: but we have al-

ready shown that no trace of it is found in the published proceedings of the Board until about a year ago. The whole course of action has ignored it; and no Regent has entered a protest against that course as illegal.

We are at a loss to know whether Mr. Choate maintains this view of the law or not. His letter of resignation (Doc. No. 7), the only record of his views that has been published, however, implies that he does. He declares that Congress matured a plan, and sketched its general features with great clearness and completeness, in the law, the substantial meaning of which is palpable and unequivocal in its terms, and that this plan has been violated, and the law in effect repealed, by the action of the Board of Regents. Yet he dates the violation, not in January, 1847, when the compromise resolutions were passed, "according to which [only] a full half of the annual income was to be eventually applied in permanence to what I deem the essential parts of the plan of Congress," but in January, 1855, when these resolutions were rescinded, and the annual appropriations required to be made specifically for each object, "according to its intrinsic importance and in compliance in good faith with the law." It is hard to say how "henceforward the discretion of the Regents, and not the act of Congress, is to be the rule of appropriation" any more after than before this change; since that discretion which Mr. Choate *allowed* had permanently cut off one half of the income from what he "deems the essential parts of the plan of Congress," while that which he *denounces* renders practicable the appropriation of the whole available income to what he contends Congress meant it should be applied. Surely it is not the discretionary power of the Regents that is here complained of, but the direction in which they were about to exercise it. But what an anomalous discretion that must be which can be exercised only in one direction!

These newly developed claims of "the library plan," however based, it is understood were eloquently argued by Messrs. Choate and Meacham before the full Board of Regents, at the annual session in January last, the Chief Justice in the chair; and the decision was strongly adverse to them. Mr. Pearce's resolutions, rescinding the so-called compromise, and requiring specific annual appropriations, were carried by votes, the first of 8 to 6; the second of 9 to 5. This was not the test question; for they might with propriety have been opposed, and indeed were opposed, by Regents who do not regard the library as the paramount interest of the Institution; while those who do so regard the library,—not on grounds of general expediency, but on the ground that the law enjoins it,—would seemingly have been constrained to vote in the affirmative. The actual vote here is very important, and we must think decisive as to the true interpretation of the law, since

the same view is implicitly maintained by both parties. The majority voted to rescind the compromise resolutions, because they had no doubt of the right, and thought it most expedient, to do so. On the other hand, Messrs. Choate and Meacham's vote to continue them surely implies that they thought them lawful! Nor were they shut up to a choice between two evils, as will be seen by the proposition which they proceeded to offer. At any rate, their vote for the continuance of a rule of action which devoted only half the income to the library, museum, and gallery altogether, is a complete answer to their argument, or at least to the argument of Mr. Meacham's report. We dismiss, therefore, without further words, a doctrine which even its originators and supporters will not stand to.

The nicer questions remain: Though the act, interpreted by itself, does not enjoin such a large average appropriation for the library, may it do so, however, in virtue either of the intention of the testator, whose will is incorporated into the law *as its intent* (as Mr. Upham justly states), or of the intentions of any of the supporters of the act in Congress, to be gathered from recollection or from the printed debates? Or, can such intentions, avowed by the proposers of a clause of the act on the floor of Congress, but not effectuated in its terms, be held,—if not exactly to modify the obvious meaning of those terms, or to necessitate a particular interpretation of them,—yet still to control somehow the exercise of the discretion which the clause confers on the Board of Regents, so as to make it the duty of this Board to carry out such intentions in a general way, by expending on the library, not indeed the whole $25,000 said to be intended, but a large though indefinite part of it? The affirmative of one or both of these propositions, expresses, we believe, the whole case of 'the library interest' in the most favorable form. The argument of Mr. Choate before the Board of Regents may be presumed to have set forth the full strength of the case. Immediately after the passage of Mr. Pearce's resolves, which repealed former restrictions and laid the whole subject open, the point of these propositions was brought directly before the Board, unincumbered by other issues, by Mr. Meacham, who moved the following:—

"*Resolved*, That a compliance in good faith with the letter and spirit of the charter of the Smithsonian Institute requires that a large proportion of the income of the Institution should be appropriated "for the gradual formation of a library composed of valuable works pertaining to all departments of human knowledge."

"The question being taken on this resolution, it was lost, as follows:

"Yeas—Messrs. Choate, Douglas, Meacham, Stuart—4.

"Nays—The Chancellor, Chief Justice Taney, Messrs. Bache, Berrien, English, Hawley, Mason, Rush, Pearce, and Totten—9."

So the question was decided against the claims of the library interest by the strong vote of 9 to 4;—the Board deliberately affirming (what no vote had ever called in question) that full power and responsibility for the apportionment of the income among the objects of the Institution is vested in itself, subject only to the terms of the law. This is a legal question, and an official decision on the construction of the law by the body responsible for its administration. A judicial decision could hardly involve a higher responsibility. Of the nine Regents who affirmed this construction, all but two, we believe, are members of the learned profession, whose business it is to construe law; most of them have borne the responsibilities of judicial functions; and at least three of them rank among the most eminent jurists of the country, viz, the venerable Chief Justice of the United States—a member of the Board *in virtue of his exalted judicial office*,—and the two ex-Attorney Generals, Messrs. Rush and Berrien. Opposed to them are Judge Douglas, Mr. Stuart of Michigan (probably a lawyer), Mr. Meacham (late a clergyman), and Mr. Choate, whose forensic reputation is certainly unsurpassed.

The responsibility and the legal and moral weight of this decision satisfy our minds, and will doubtless satisfy the public. Had its grounds been given (as they would have been in case of a like judicial decision), it would be great presumption in us to continue our critical analysis; and as it is, our remarks shall be brief.

1. As to the intentions of the founder of the trust, and of individual promoters of the Act of Congress creating the establishment to execute the trust: which of the two are to be regarded, or most regarded, in the Construction of the law? Of course, the former; because the declared purpose of the Act is to execute the founder's will: the latter are of no consequence except as ancillary to the former, and of no force except as they may be embodied in the law. The law can have no intentions that are not expressed in its provisions or preamble: and the expressed intention of this law is, to provide for the faithful execution of the will of Smithson.

It is plain enough that the law does not in terms provide that the establishment it creates shall be mainly a library. Does it intend this, however, in virtue of any expressed or implied intention of the founder? Is it even supposable that Smithson had a library in view? The bequest was made "to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men." It has often been said, that any man who regarded a library as the obvious, or the most important means for the increase and diffusion of knowledge among men would have been sure, in such a case, to have written '*library*' in place of '*institution*,' or

'*establishment.*' But even this does not fully express the point. Had Smithson been a book-collector, and left the will he did, who would have allowed that a library was intended? Is it not, however, equally preposterous to place this construction on these words of a man who had no library at all; whose twenty-seven published memoirs, it has been truly said, might every one of them have been prepared, and probably were prepared, without consulting a library; who during his lifetime labored to increase knowledge solely by original researches in chemical and physical science; and who bequeathed his fortune so that it should continue to furnish the means of increasing and diffusing knowledge in the world after his removal from it? In our minds, the only question is, whether a diversion of this fund mainly toward a library be an allowable use of it; and whether, in the appropriating clause of the act which, by fixing so high a maximum, would legalise this use of it, Congress did not unduly stretch its prerogative over the trust. We rest, however, in the hope and expectation that this clause will continue to be construed by its terms, and by the intention of the founder,—cordially agreeing as we do with the whole Judiciary committee of the Senate, that "this section cannot, by any fair construction of its language, be deemed to imply that any appropriation to that [maximum] amount, or nearly so, was intended to be required. It is not a direction to the Regents to apply that sum, but a prohibition to apply more; and it leaves it to the regents to decide what amount within the sum limited can be advantageously applied to a library, having a due regard to the other objects enumerated in the law." And the Judiciary Committee proceed to say:

"Indeed, the eighth section would seem to be intended to prevent the absorption of the funds of the Institution in the purchase of books. And there would seem to be sound reason for giving it that construction; for such an application of the funds could hardly be regarded as a faithful execution of the trust; for the collection of an immense library at Washington would certainly not tend 'to increase or diffuse knowledge' in any other country, not even among the countrymen of the testator; very few even of the citizens of the United States would receive any benefit from it. *And if the money was to be so appropriated, it would have been far better to buy the books and place them at once in the Congress library.* They would be more acceptable to the public there, and it would have saved the expense of a costly building and the salaries of the officers; yet nobody would have listened to such a proposition, or consented that the United States should take to itself and for their own use the money which they accepted as a trust for 'the increase and diffusion of knowledge among men.'"

2. As to the intentions of members of Congress who promoted the passage of the bill into a law in its actual form:—let all that can be justly claimed for the library plan on this ground be con-

ceded. These claims are very elaborately presented in Mr. Meacham's minority report (Doc. No. 5), to which we refer our readers. They amount substantially to this—First: Specific plans of operation, either contained in the original bill, or moved in amendments,—some of them prescribing the same 'active operations' that the Institution has undertaken,—were voted out. Well and wisely done, no doubt. No one specific plan, no enumerated set of operations, to be fixed once for all, could obtain the vote of a clear majority. Where various conflicting schemes are advocated, it is not difficult to outvote any one. The question turns, upon what is put in its place. We gather from 'the parlimentary history' of the Act, that the real majority did not wish to anticipate experience, and fix details, but to found an Institution, subject to a few general requirements; to give it able and responsible managers; to devolve on them the duty of reconciling as far as they could the conflicting plans so vehemently urged by several different parties, and to apportion the income among the specified objects according to the dictates of their best judgment, enlightened more and more by experience. So say the Judiciary Committee of the Senate.

"It is very evident, by the law above referred to, that Congress did not deem it advisable to prescribe any definite and fixed plan, and deemed it more proper to confide that duty to a Board of Regents, carefully selected, indicating only in general terms the objects to which their attention was to be directed in executing the testator's intentions.

"Thus, by the fifth section, the Regents were required to cause a building to be erected of sufficient size, and with suitable rooms or halls, for the reception and arrangement, upon a liberal scale, of objects of natural history, including a geological and mineralogical cabinet, also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms. It is evident that Congress intended by these provisions that the funds of the Institution should be applied to increase knowledge in all of the branches of science mentioned in this section—in objects of natural history, in geology, in mineralogy, in chemistry, in the arts,—and that lectures were to be delivered upon such topics as the Regents might deem useful in the execution of the trust. And publications by the Institution were undoubtedly necessary to diffuse generally the knowledge that might be obtained; for any increase of knowledge that might thus be acquired was not to be locked up in the Institution, or preserved only for the use of citizens of Washington, or persons who might visit the Institution. It was by the express terms of the trust, which the United States was pledged to execute, to be diffused among men. This could be done in no other way than by publications at the expense of the Institution. Nor has Congress prescribed the sums which shall be appropriated to these different objects. It is left to the discretion and judgment of the Regents."

And further,

"No fixed and immutable plan, prescribed by law or adopted by the Regents, would attain the objects of the trust. It was evidently the in-

tention of the donor that it should be carried into execution by an Institution, or Establishment, as it is termed in his will. Congress has created one, and given it ample powers, but directing its attention particularly to the objects enumerated in the law; and it is the duty of that Institution to avail itself of the lights of experience, and to change its plan of operations when they are convinced that a different one will better accomplish the objects of the trust. The Regents have done so, and wisely, for the reasons above stated."

But, secondly, Mr. Meacham contends that the appropriating clause, fixing the maximum expenditure on the library at $25,000, having been introduced into the bill with the intention, avowed by its advocates in Congress, of making the Institution "almost entirely a library," therefore, "it seems impossible to doubt that Congress in the charter of the Institution intended to indicate that the library . . . should receive about $25,000, a year, and that the museum, laboratory, and lectures should receive only about $6000 a year." This argument, somehow, did not convince the Chief Justice and the rest of the lawyers; who probably reflected, —1, that the intention of these advocates might not be the intention of the majority who voted for the clause, and still less the intention of Congress; many may have preferred to admit the clause, although involving the *possibility* of the diversion of the bequest to a library, rather than by its rejection to hazard the passage of the otherwise satisfactory bill into which it was inserted:—2, that these advocates well knew how to express their intention in the terms of the clause, had they been so minded, having a pattern in the bill that passed one House the year before, in which $20,000 was *required* to be annually expended on the library, and that they would have so worded the present clause of the 8th section had they felt able to carry it;—and, 3, that, since it could no better be, they were content with the opportunity which Congress (we think unadvisedly) afforded them of accomplishing their end in the Board of Regents, whenever they could convince this body that a library was a proper, and the best practicable instrument for fulfilling the purpose of Smithson's bequest. This they have not yet succeeded in doing. We cannot believe that they did not well know the difference between the mandatory appropriating clause in the former bill, and the permissive one of the present act. But even if we suppose they did think that their intention was by some implication fixed upon the clause in question, is that intention to override or qualify its terms? An exactly parallel case would have arisen had the Board of Regents adopted (instead of rejecting, 9 to 4) Mr. Meacham's resolution appended to his report, and quoted above; viz, that a compliance with the law requires that *a large proportion* of the income should be appropriated for a library; the intent and meaning of which, in the mover's mind, as expressed in the body of the report, being

that this "large proportion" is an average of $25,000 a year. Would the Board have adopted the proposition with Mr. Meacham's understanding of it, or merely in its obvious terms? Who ever heard of a legislation which devolves a discretion over the amount of an appropriation upon a Board of trustees, but yet tacitly assumes that this discretion shall not be exercised at all; or shall be exercised only within some narrow and undefined limits, to be gathered, not from the act itself, but from the intentions and motives of a few of its promoters; which again are to be gathered from their speeches in Congress! And this on a point involving the construction of a will and testament, in which parole evidence is of no account.

Such are some of the considerations which render manifest to us the correctness of that construction of the law which the Regents have all along acted upon, and have lately (when impugned) formally affirmed. It has been re-affirmed and sanctioned, in the fullest terms, by the unanimous voice of the Judiciary Committee of the Senate (which is always chosen from among the ablest jurists of that body), when referred to them by a motion of the Hon. Mr. Clayton, made on the express ground that, "if the Regents are right in the interpretation they have given to the law, they should be sustained by the judgment of the Committee and the judgment of the Senate." It has been adopted in the report of one part of the special committee raised by the House; and it is scarcely attempted to be controverted by the other.

We have devoted our attention principally to the question of the *right* rather than the *expediency* of the actual policy of the Smithsonian Institution; because it is on this that the force of the charge falls; and because, for the want of a proper analysis of the case, some friends of this policy have been in doubt whether it is fully compatible with the terms and spirit of the law. That once settled, the propriety of the course pursued by the Institution need not be argued to the readers of this Journal. It is heartily approved by the whole scientific interest of this country; it appears to be favorably regarded by a large share of our best scholars and literary men. Its earnest endeavors to do that which cannot be done, or so well done, by existing institutions; to turn to utmost account the means of diffusing and increasing knowledge at its disposal; its refusal to stereotype one plan, and that the most localized, for all circumstances and times, and to give to place or "party what was meant for mankind," are most consonant to the spirit and presumed intentions of the founder, and are, we believe, regarded with high satisfaction by the learned world. Spontaneous and ample evidence of this comes day by day into our hands. We think the Regents do well, therefore,—instead of expending all or most of their means

upon a universal library at Washington (thus providing from a foreigner's bequest for a want which the General Government may be expected to supply, and are now supplying quite as largely as the whole income of Smithson's fund would),—to accumulate by degrees a "library of valuable works pertaining to all departments of human knowledge," with special reference to those most needed by the investigator and enquirer, and not otherwise accessible, and to journals and transactions of all learned societies, in which the principal advances in knowledge are recorded; to promote original researches in every department of enquiry; to publish and freely distribute important "contributions to knowledge," as well as reports giving an account of the discoveries in science from year to year wherever made; to organise and direct, and whenever necessary conduct, an extended and well-directed system of meteorological observations over the length and breadth of the country; to aid scientific explorations, especially such as tend to bring to light the natural resources and productions of America; to gather and arrange collections, especially of our own natural productions, and such as illustrate and verify the publications and researches made by the Institution, or under its auspices; to solve important experimental problems in chemistry, physics, &c., or to furnish instruments and facilities for so doing; and to continue the system of general international scientific and literary exchange, by which every institution of learning and science in the United States is enabled to carry on its correspondence, transmit its publications to all similar societies in any other part of the world, and to receive theirs in return, almost without expense,—an economising of the feeble and scattered scientific means of the country, which largely contributes to the diffusion and increase of knowledge. Some details respecting this good work of the Smithsonian Institution would be interesting and perhaps new to many of our readers. But our space is too limited to give them.

We cannot, therefore, attempt an analysis of the two reports which have emanated from the special committee raised in the House of Representatives, the latest documents on our list. Nor is this necessary; for they throw no new light on the question with which we have here been mainly occupied. We will barely notice, in passing, one or two statements in the report (Mr. Upham's) which, to a considerable extent, sustains views adverse to those of the Board of Regents. There are three new suggestions in this report, of whose value our readers can form their own opinion. Two of them look to alterations of the law, viz: that the executive affairs of the Institution should be administered by two officers of equal authority; and that the Institution itself might be made a bureau under a department of the General Government, instead of remaining as now, a trust administered by an independent establishment. The third propounds a novel con-

struction of the words of Smithson's will, viz, "the increase and diffusion of knowledge among men," which we give in full, to exemplify the straits those are reduced to who contend that the founder's design would really be attained by a library at Washington. Says Mr. Upham:

"The words 'among men' were used merely to corroborate the idea expressed by the word 'diffusion.' They do not necessarily imply that the Institution should confine itself to world-wide operations. The word is not, as some seem to suppose, 'mankind' but 'men;' and he diffuses knowledge 'among men' as truly, and in as full a sense, when he enlightens the minds of his neighbors, as of persons at the very farthest pole."

Really would not this construction be about as good as now, even if the word "mankind" had been used, instead of "men?" And although the words "do not necessarily imply that the Institution should *confine* itself to world-wide operations,"—an odd sort of confinement indeed,—do they not imply that it should *extend* itself "to world-wide operations?" Again:

"The word 'INCREASE' is held by some of the zealous combatants in the Smithsonian controversy to be identical with 'DISCOVERY.' The idea seems to be that knowledge can only be *increased* by the *discovery* of *new* truth. This is an arbitrary and untenable position. A mind experiences an increase of knowledge if it knows more than it did before, although all the ideas it has received may be in the commonest textbooks. There has been an increase of knowledge in the school, or the congregation, or the lecture-room, if ideas not before known to them have been received into the minds of the hearers, even; indeed it matters not if those ideas have been recorded for thousands of years in languages, classical or sacred, that have been dead long ago. Knowledge has been increased if one mind has received more, whether it be new or old truth. The language of Smithson is perfectly simple, and in its natural sense covers the whole ground; it includes but does not require *new* truth. Truth discovered a thousand years ago is as good as truth discovered yesterday. Knowledge embraces it all alike, and Smithson's object was to carry knowledge where it was not before, and to increase it where it was; to spread it over a wide area and to a greater depth."

We would only ask; if the communication of known facts and ideas from one to another constitutes "*the increase of knowledge*" in the sense of Smithson's will, then what did Smithson intend, and what does Mr. Upham mean, by "*the diffusion of knowledge?*"

To show the impropriety of publishing memoirs, like those in the Smithsonian Contributions to Knowledge, a fear is expressed that favoritism will be practiced, injury inflicted on some individuals through a condemnation of their treatises by "a secret tribunal," resentments enkindled, and perhaps important discoveries be suppressed. With more reason might the same complaint be

made against this Journal: for a greater amount of acceptable matter is offered us than we can publish, and we are obliged to make our own selection; while the Smithsonian Institution, in each case, calls in the aid of the ablest experts it can find in the country, and holds their published names responsible for their judgment. And the author of a rejected memoir could surely have his treatise referred to a second committee, or bring it before the world in some appropriate periodical journal, or give it to some one of our learned societies; which are willing enough, and generally able, to publish any such really important contribution to knowledge, if not accompanied by numerous and costly plates, and always to publish as much as is requisite to bring the new discovery or new ideas before the world. The publication of full details and of the needful pictorial illustrations is indeed important; and herein the Smithsonian Institution is of great use, by publishing valuable memoirs with illustrations which are beyond the means of the author and of our learned societies: but this service is rendered, not so much to the author, who may always bring out the substance of his views so as to secure the merit of discovery, as to the public he addresses, who are thereby furnished with the means of fully comprehending the case, and drawing their own conclusions from his data. Mr. Meacham, adopting an opposite view, expresses the opinion that, without the agency of the Institution, the present Smithsonian memoirs "would all have been published, some in journals or transactions of societies, and some in separate books,—not, perhaps, in so elegant a style." This, we know, is not the case as respects some of the larger memoirs; and if the rest had been so published, an equal quantity which the principal learned societies and journals have in the meantime given to the world could not have appeared.

The suggestion that the Institution might be administered as a bureau of the Department of the Interior, strikes us as by no means happy, nor compatible with the nature of the trust,—a private foundation for the public benefit, to be administered not by the executive government, but by an establishment for the purpose. We think that neither this, nor Mr. Upham's extraordinary suggestion, that the Institution "would be found to work most auspiciously" under two coördinate executive officers, is likely to be seriously entertained. Nor can we agree with him that the present system is faulty, and "necessarily surrounded with very great difficulties." The whole committee bear witness to the "zeal, sincerity, integrity, and high motives and aims" of "those who suggested and have labored to carry out that system." Let such administrators have opportunity to give that system a fair trial, free from the embarrassments of "an antagonistic position" on the part of any one charged with the duty of car-

rying their views into effect, before any actual or anticipated practical difficulties are charged upon the present organization.

It does not fall in our way to discuss the question, whether the Secretary possesses the right to dismiss, for assigned reasons, a subordinate for whose official conduct he is responsible to the Board. Let it suffice that the Board of Regents had formally declared this power to belong, in their opinion, to the Secretary (the Chief Justice, then absent, spontaneously adding his concurrence), and this in direct reference to its application in the only case of its exercise that could admit of a doubt. We would not question the motives of the late Assistant Secretary in charge of the library, nor lay down our rule of conduct for others. But we ourselves should not think it becoming and proper to continue to hold and receive the emoluments of a responsible appointment, under a Secretary against whom we had made charges, and after those charges had been pronounced unfounded by the Board, nor while our sense of duty to the public impelled us to labor for the subversion of the recognised policy of our employers, and to defeat the re-election of an *ex-officio* member of the Board; and we should think it passing strange to be allowed to do so. The statements of the two reports from the House Committee equally and effectually dispose of the case of Mr. Blodgett, which has also been paraded as a grievance in the public prints.

The testimony quoted, in the last paragraph but one, from Mr. Upham's report, is a sufficient answer to the allegation and insinuation of unworthy motives with which most of the articles in newspapers and magazines, assailing the Regents and the Secretary of the Institution, have abounded. In one notable instance, a writer in a popular magazine does not spare even the memory of the noble founder, but calls him a "poor, proud, insignificant dabster in the chemistry of tears, and the mathematics of *rouge et noir*," and scouts the idea of paying any regard to his will in the construction of the law for executing the bequest he entrusted to our country's guardianship.

The attempt to form a spurious public opinion by appeals to men's prejudices, and by the iteration of perverted statements cannot be long successful. Public opinion may have been misled in some quarters for a season; but juster views are sure to prevail,—we think are generally prevailing already.

NOTE.—Just as this sheet is going to press we have received a copy of the official edition, just issued, of the two Reports from the House of Representatives' Select Committee (Mr. Upham's and Messrs. Witte and Taylor's), with various documents annexed. A hasty perusal of these documents shows nothing requiring any modification of the statements and argument of the present article; although there is much that might call for remark. We are informed on good authority that a part of these papers had never been before the committee, but must have been appended after its latest sessions, without the knowledge or consent of a majority of its members; while some documents that were before the committee are omitted! [May 10th.]

ART. II.—*Description of a new species of Clathropteris, discovered in the Connecticut Valley Sandstone;* by E. HITCHCOCK, Jr., M.D.

THE position of the sandstone of the Connecticut valley is of no small interest and importance to the Geologist, and has never been positively determined. This is mainly owing to the fact, that with the exception of the footmarks, very few well characterised fossils have been discovered in this formation.

During the last summer a specimen of a fossil fern was found in the sandstone of Mt. Tom, in Easthampton, Mass., which seems without doubt to belong to the genus *Clathropteris* of Brongniart. Previous to this discovery several specimens of this genus had been found, and are now in the Cabinet of Amherst College; but none so well defined as to indicate the genus to which they now undoubtedly seem to belong.

The generic description of this plant is thus given by Brongniart: "Folia pinnatifida, pinnulis elongatis, nervo medio valido percursis: nervis secundariis pinnatis parallelis simplicibus, nervis transversalibus unitis, areolas subquadrangulares efformantibus, nervulis tenuoribus reticulatis notatas," who considers the quadrangular spaces formed by the veins and veinlets as the decisive characteristic.

Only one species is as yet described, "meniscioides," named from the resemblance of the spaces between the veins, to a meniscus.

To the East Hampton species we have provisionally attached the name *Clathropteris rectiusculus*, from the supposed absence of the meniscoid character. We offer the following description:—

Clathropteris foliis profunde pinnatifidis, pinnulis rachi adnatis, inter se discretis, oblongis, bipedalibus vel majoribus; nervis secundariis rachi subperpendicularibus, 4 lineis distantibus; nervis transversalibus rectiusculis vel leviter arcuatis clathratis, nervulis tenuissime reticulatis, superficie foliorum planiuscula.

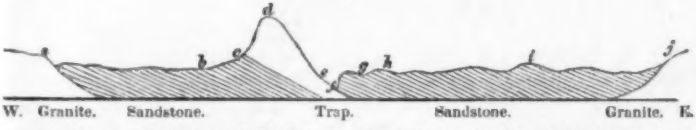
Geological position of the European Species.—In 1828, Adolph Brongniart regarded this fern as very characteristic of the Lias sandstone, which (fern) had been discovered in these localities. and he thus speaks concerning it: "This fossil fern, so remarkable in its structure, and by its well marked relations to a small group of living ferns, is not less so in a geological point of view. In reality since I have found it at Hoer in the arkose formation, all the fossil plants of which compel me to refer it to a position between the lias and the chalk, this same plant has been found in two other localities where the geological position is well determined; at Mt. St. Etienne near La Marche in the Vosges, in a

formation which Elie De Beaumont considers as certainly belonging to the lias; and at Pouilly in Auxois, by Messrs. Bonnard and Beaumont in a bed of arkose of the same epoch of formation; both of which go to prove that this plant is one of the most important characteristics of the lias, and it is probable that it will appear again in more localities where this formation exhibits itself."*

Nine years later, Prof. Bronn, in his Lethæa Geognostica, modifies these statements of Brongniart as follows; "as yet no determinate position has been found for the sandstone of Hoer in Scania, which Adolph Brongniart includes in his common group of keuper, variegated marl, and lias, and Clathropteris meniscioides appears therein as well as in the Lias sandstone of La Marche in the Vosges, along with Equisetum columnare, which latter sandstone formation however (that of the Vosges) as it appears belongs to the keuper, in which Alberti again notices this fern in Neuen Welt near Basle."†

In 1849, in his Index Palæontologicus, Bronn places the Clathropteris both in the Lias and the Keuper.‡

1.



a, Southampton lead mine; *b*, Easthampton Seminary; *c*, locality of Clathropteris; *d*, Mt. Tom; *e*, locality of foot-marks; *f*, Connecticut river; *g*. Mt. Holyoke Seminary; *h*, Hadley; *i*, Granby; *j*, Rock Rimmon.

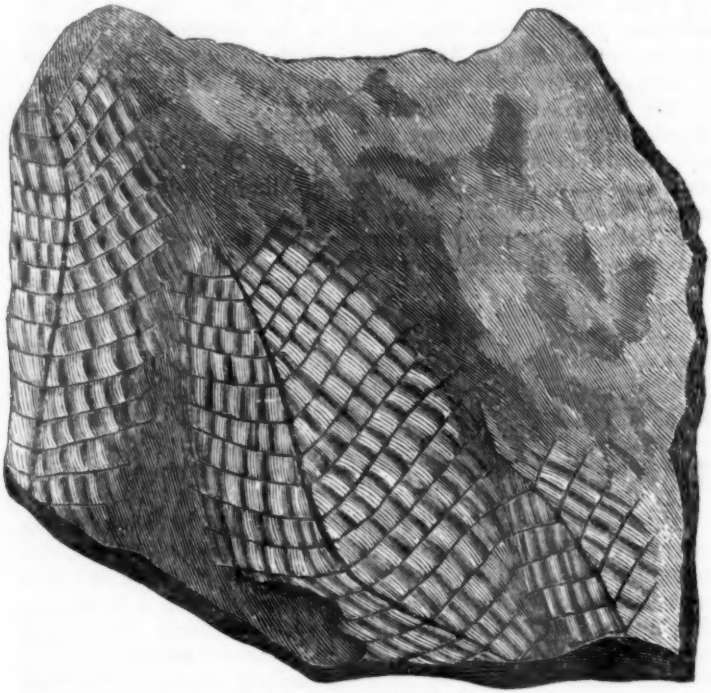
The Clathropteris of East Hampton is found in a coarse reddish sandstone, on the west face of Mt. Tom. The upper part of this mountain is trap, beneath which the sandstone crops out with an easterly dip of about 25°. The sandstone has a southeasterly dip across the whole of the Connecticut valley. That east of the trap range is made up of finer materials, and is of a more slaty character than that on the west. Bassett's Quarry, where this fern occurs is somewhat west of the middle of the valley, as may be seen in the annexed section. In this section lately measured by my father—Dr. Hitchcock of Amherst College—it is found that the thickness of the sandstone east of Mt. Tom is more than 8,000 feet, and that on the west, or below Mt. Tom is nearly 5,000 feet. Supposing one half of this thickness to be accounted for by original deposition on an inclined surface, there will still remain a thickness of some thousands of feet both above and below the locality of the fern. A thickness equally great for the sandstone of the Connecticut valley in measuring another sec-

* Brongn. Veg. Foss., p. 131 and 132.
† Lethæa Geognostica, vol. i, p. 140.
‡ Index Palæont., vol. ii, p. 22.

tion across the valley at Turner's Falls, 30 miles north of East Hampton.

Fig. 2 exhibits portions of several fronds or perhaps only pinnules of the East Hampton fern. Some of these pinnules—although much broken at their extremities—are nearly a foot long, and 4 inches wide, so that probably they were originally at least one foot and a half in length.

2.



Clathropteris rectiusculus.—H.

The pinnule on the left hand side of the figure, shows the delicate secondary veins, which are usually obscure and therefore this fern has not heretofore been recognised. I have presented to the Cabinet of Amherst College, a large specimen from E. Hampton containing numerous isolated fronds, and in one place showing a large number most distinctly radiating from a center, like the tree ferns of tropical regions. These radiating fronds are broken off at their extremities, being only 4 to 6 inches in length. The specimen which is figured measures nine by thirteen inches, and is in the Cabinet of the Williston Seminary at East Hampton.

In the Cabinet of Amherst College is a fine specimen of a radiating Clathropteris (that is its apex) from the quarry of Roswell Field in Gill, Mass., and although nearly one quarter of the circle is lost, yet as many as 17 distinct fronds can be counted radiating from one stem. Although the reticulated character of this specimen is rather obvious, it was not noticed till the more recent specimen from East Hampton was discovered.*

In the same cabinet is another obscure specimen of the Clathropteris from the banks of Connecticut river in Montague, two miles southwest of Field's quarry, a sketch of which is given in my father's final report on the Geology of Massachusetts, vol. ii, p. 452.

The specimens from Gill and Montague occupy nearly the same position in the sandstone as the one at E. Hampton, although they are on the east side of the trap range.

Conclusions.—The above facts make it almost certain that a species of Clathropteris occurs in the sandstone of the Connecticut valley not far from its center, measuring across the strata, and near to the interstratified beds of trap both above and below. Now since this fern is found in Europe only in the upper part of the Trias, and the lower part of the Lias, it is very probable that it occupies the same geological position here. If so we ascertain the existence of a zone of rock in the Connecticut valley not far from the junction of the Lias and the Trias. And since two measurements of sections across this valley show a thickness of sandstone strata both above and below this zone thicker than the Lias and the Trias of Europe, the probability seems very strong, that the equivalents of both of these rocks exist here, and not improbably some others both older and newer.

If we can rely with confidence upon this geological zone, it will form a convenient starting place for tracing out other older and newer formations.

It will be seen that while the above conclusions sustain the opinion lately advocated with much ability by Prof. W. B. Rogers (Am. Jour. Sci. Jan., 1855, p. 123), that the Lias sandstone exists in the valley of the Connecticut, it makes the opinion also probable—long since advanced by my father that the Trias also exists here.

The radiations in the specimen above noticed appear to be distinct and entire fronds, and so resemble those on fig. 2, as to make it probable that the latter also are fronds and not pinnules.

* I have reason to suppose that this specimen has been mistaken for a Zamia.

ART. III.—*On the Periodical Variations of the Declination and Directive Force of the Magnetic Needle;* by W. A. NORTON, Professor of Civil Engineering in Yale College.

(Continued from vol. xix, page 211.)

IN the calculations made on pp. 207, 208, of the relative effects of the ecliptic and radial currents, no account was taken of the possible effects of *residual* currents, that is, of the gradually subsiding currents which may succeed those which are directly developed by the sun's action. I now propose to inquire into the nature and extent of their action, to make due allowance for it, and to apply additional numerical tests to the theory under examination. But I would previously remark that there is no occasion to distinguish the action of the primary from that of the secondary ecliptic currents, so called, at the equinoxes, in considering the entire effect in the interval from 6 A. M. to noon or from noon to 6 P. M. (See p. 205.) It is only when we are comparing the hourly variations that this distinction need be made. In addition to this qualification of certain statements made on p. 205, it should be observed (although the correction is of little consequence) that the secondary current, previous to 6 A. M., is in fact equally inclined to the meridian at the two equinoxes.

If we confine our attention to the entire semidiurnal interval just mentioned then the ecliptic and radial currents tend, both in the forenoon and afternoon, to deflect the needle in the same direction, at the autumnal equinox, but in opposite directions at the vernal equinox. Disregarding, for the present, the residual currents, let r denote the semidiurnal effect of the radial and e that of the ecliptic currents at the equinoxes; then taking the data from Table I, (p. 194,) we have, for the forenoon, $r+e=8'{\cdot}65$, $r-e=5'{\cdot}67$, and therefore $r=7'{\cdot}16$ and $e=1'{\cdot}49$. For the afternoon, $r+e=7'{\cdot}89$, $r-e=2'{\cdot}78$; hence $r=5'{\cdot}33$, and $e=2'{\cdot}55$. The two values of r differ from each other because of the action of residual currents. As a first approximation to the determination of the semidiurnal effect of the residual radial currents, let R = diff. of effects of the primary radial currents set in circulation at the beginning and end, respectively, of either the forenoon or the afternoon interval, and x = effect of the residue of all the radial currents developed during either interval; then $R+x=7'{\cdot}16$, $R-x=5'{\cdot}33$, $R=6'{\cdot}24$, $x=0'{\cdot}92$. Making the same calculations with the data from Table II, (p. 208,) we have, for the forenoon, $r=7'{\cdot}59$, $e=1'{\cdot}91$; for the afternoon, $r=4'{\cdot}50$, $e=2'{\cdot}03$: also $R=6'{\cdot}04$, $x=1'{\cdot}54$. $e=\frac{2'{\cdot}03+1'{\cdot}91}{2}=1'{\cdot}97$. The average of the two values of e found above, from Table I, is $2'{\cdot}02$. The difference of the two determinations is only $0'{\cdot}05$.

At the summer solstice, we have, for the forenoon $R+(e+x)=9'{\cdot}69$, for the afternoon, $R-(e+x)=3'{\cdot}80$; whence $R=6'{\cdot}74$, and $e+x=2'{\cdot}94$. The value found for R at the equinoxes is $6'{\cdot}24$, and of x, $0'{\cdot}92$; hence to find x at the summer solstice, we have the proportion $6'{\cdot}24 : 6'{\cdot}74 :: 0'{\cdot}92 : x = 0'{\cdot}99$. Thus $e=2'{\cdot}94-x=1'{\cdot}95$. If we make use of Table II, we obtain $R=7'{\cdot}01$, $x=1'{\cdot}79$, $e=2'{\cdot}95-x=1'\,16$. At the winter solstice, in the forenoon, $R+x-e=1'{\cdot}15$, in the afternoon $R-x+e=1'{\cdot}62$; whence $R=1'{\cdot}38$, $e-x=0'{\cdot}23$. To find x, $6'{\cdot}24 : 1'{\cdot}4 :: 0'{\cdot}92 : x=0'{\cdot}21$. $e=0'{\cdot}23+x=0'{\cdot}44$. Table II gives $R=2'{\cdot}56$, $x=0'{\cdot}65$, $e=x-0'{\cdot}23=0'{\cdot}42$. It is to be observed that at the two solstices the ecliptic currents cross the meridian at right angles, at the hour of noon, and that therefore the values of e just determined show the effects of the ecliptic currents on the declination at 6 A. M. or at 6 P. M. At the equinoxes these currents have but little if any sensible effect on the declination at 6 A. M. and 6 P. M., but cross the meridian obliquely at noon, and therefore the value of e at the equinoxes shows the effect of the ecliptic currents on the declination at noon.

Having obtained these first approximations let us now proceed to a more minute investigation;—taking into account the residual ecliptic as well as radial currents.

Let R= deflection, in the semidiurnal interval, that would result from the variation in the intensity of the radial currents.

E= effect, during same interval, of the variation of the ecliptic currents.

r= effect at noon of the residue of the radial currents developed during the forenoon.

e= effect at noon of the residue of the ecliptic currents developed during the forenoon.

r'= effect at 6 P. M. of the residue of the radial currents developed during the afternoon.

e'= effect at 6 P. M. of the residue of the ecliptic currents developed during the afternoon.

mr= portion of residual radial currents at noon that pass off in the afternoon.

ne= portion of residual ecliptic currents at noon that pass off in the afternoon.

$r=r'$ (nearly); $e=e'$ (nearly).

At the Equinoxes; in the forenoon, we have (from Table I); Aut. Eq., $R+r+E-e=8'{\cdot}65$; Ver. Eq., $R+r-E-e=5'{\cdot}67$. Hence $E=1'{\cdot}49$, $R=7'{\cdot}16+e-r$. $r>e$, whence $R<7'{\cdot}16$. For the afternoon the equations are, Aut. Eq., $R+E+mr-r'-ne-e'=7'{\cdot}89$; Ver. Eq., $R-E+mr-r'-ne-e'=2'{\cdot}78$. From which we obtain $E=2'{\cdot}55$, $R=5'{\cdot}33-(mr-r'-ne-e')=5'{\cdot}33+r(1-m)+e(1+n)$, (nearly). No account has been taken of the residual

action at 6 A. M. of the nocturnal currents, but this must be very slight, especially as the effect of the ecliptic currents is reduced to zero at that hour. Since this action must be the same at one equinox as at the other it can have no effect on the value of E. Its only effect will be to increase slightly the value of R. For the determination of r, we have the equations $R=5'{\cdot}33+r(1-m)+e(1+n)\ldots(1)$, $R+r=7'{\cdot}16+e\ldots(2)$; whence $r=\frac{1'{\cdot}83-ne}{2-m}$ (3).

At the *Summer Solstice;* for the forenoon, $R+E+r-e=9'{\cdot}69$ (4), for the afternoon, $R-E+mr-r-ne-e=3'{\cdot}80\ldots$ (5). Whence $E=2'{\cdot}94-\frac{r(2-m)+ne}{2}\ldots(6)$, $R=6'{\cdot}74+\frac{e(2+n)-mr}{2}$ (7). To take account of the residual action of the nocturnal currents, at 6 A. M., let x denote the effect of the residual radial currents at that hour, and y that of the residual ecliptic currents: x and y will each tend to diminish the declination (West), and therefore, supposing these currents to pass off entirely during the forenoon, instead of equation (4) we have $R+E+r-e+x+y=9'{\cdot}69\ldots(8)$, and instead of (6) $E+y=2'{\cdot}94-\frac{r(2-m)+ne}{2}+\frac{y-x}{2}$ (9); also $R=6'{\cdot}74+\frac{e(2+n)-mr}{2}-\frac{y+x}{2}\ldots(10)$. At the Winter Solstice, $R-E+r-e=1'{\cdot}15\ldots(11)$, $R+E+mr-r-ne-e=1'{\cdot}62\ldots(12)$; and thus $E=0'{\cdot}23+\frac{r(2-m)+ne}{2}\ldots(13)$, and $R=1'{\cdot}38+\frac{e(2+n)-mr}{2}\ldots(14)$. If we take account of the residual nocturnal currents in action at 6 A. M., $E+y=0'{\cdot}23+\frac{r(2-m)+ne}{2}+\frac{y-x}{2}\ldots(15)$, $R=1'{\cdot}38+\frac{e(2+n)-mr}{2}+\frac{x+y}{2}$ (16).

If we add together eqs. (6) and (13), accenting the letters of eq. (13), we obtain $S=E+E'=3'{\cdot}17-\frac{(2-m)(r-r')+n(e-e')}{2}$ (17). Since E and E′ represent the effects of the ecliptic currents on the declination at the hour of 6 A. M. at the summer and winter solstice, respectively, this formula should give the deflection of the needle at that hour, due to the ecliptic currents in the interval between the solstices. To take account of the residual nocturnal currents we must introduce into the formula the terms $\frac{y+y'}{2}-\frac{x+x'}{2}$. The exact value of this expression cannot readily be found, but we know that it must be a very small quantity. In the calculations which follow it is neglected.

The determination of the deflection produced by the ecliptic currents, in the interval between the solstices, from formula (17) admits of verification. We have seen (p. 201), that, for the hour of 6 A. M., the entire deflection in this interval is the sum of the effects of the ecliptic and radial currents, and that for the hour of 6 P. M. the deflection during the same interval is the difference of the effects of these two systems of currents. Now let E= effect of the ecliptic currents at 6 A. M. or 6 P. M.; R= effect of radial currents at the same hours; e and r the effects of the residual ecliptic and radial currents, respectively, at 6 A. M.; and e', r', the effects of the two systems of residual currents which are in action at 6 P. M. Then, (by Table I,) $E+R+e+r=4'\cdot90$, $E-R+e'+r'=1'\cdot46$; whence, $E+e=3'\cdot18-\frac{e'-e}{2}-\frac{r+r'}{2}$ . . (18).

It will be seen, when we come to discuss the variations of the horizontal force, that the intensity of the ecliptic currents is twice as great, at the hour of noon, at the winter as at the summer solstice; but on the other hand these currents pass off, from the vicinity of the zenith, about 4½ P. M. It is probable, therefore, that the effect of the residual ecliptic currents at 6 P. M. is nearly the same at the two solstices, and accordingly that e', which is the difference between the individual effects at the solstices, is a small quantity in comparison with r'. $\frac{e}{2}$ and $\frac{r}{2}$ are also small quantities. If we neglect $\frac{e'-e}{2}$ and $\frac{r}{2}$, we have $D=E+e=3'\cdot18-\frac{r'}{2}$ (nearly) . . . (19). This formula should give nearly the same value for the deflection as formula (17).

The following Table contains the results given by the formulæ which have just been investigated, corresponding to certain assumed values of m and n.

	Equinoxes.				Summer Solstice.				Winter Solstice.				S	D
	R	E	r	e	R	E	r	e	R	E	r	e		
$m=1$, $n=1$	6'·24	2'·02	1'·37	0'·46	6'·40	2'·14	1'·37	0'·23	1'·57	0'·50	0'·31	0'·23	2'·64	2'·65
$m=0$, $n=0$	6·55	2·02	0·92	0·31	6·89	2·02	0·92	0·15	1·53	0·43	0·20	0·15	2·45	2·82
$m=\frac{1}{2}$, $n=\frac{1}{2}$	6·43	2·02	1·10	0·37	6·69	2·07	1·10	0·18	1·54	0·46	0·25	0·18	2·53	2·76
$m=\frac{3}{4}$, $n=\frac{3}{4}$	6·35	2·02	1·22	0·41	6·56	2·10	1·22	0·20	1·55	0·47	0·27	0·20	2·58	2·70

The process of calculation is as follows: It was first assumed that at the equinoxes the semidiurnal effects of the residual radial and ecliptic currents, (r and e), bear the same ratio to each other as the corresponding direct effects of the two sets of currents, (R and E), and as we have seen (p. 26) that at the equinoxes R=3E (nearly), we have also $r=3e$. Introducing this value of r into equ. (3), we obtain the value of e, and thence the value of r. R has nearly the same value at the summer solstice as at the

equinoxes; the same is therefore true of r. It will be seen in the sequel that the semidiurnal effect of the ecliptic currents, on the horizontal force, is twice as great at the equinoxes as at the summer solstice; from which it may be inferred that the actual intensity of these currents, and therefore also the effect of the residual ecliptic currents, on the declination, (e) is twice as great at the one epoch as at the other. In the discussion of the variations of the horizontal force it will also appear that the intensity of the ecliptic currents is nearly the same at the winter solstice as at the equinoxes, but as these currents are in action at the zenith during only a little more than two-thirds of the semidiurnal interval at the winter solstice, I conclude, (taking all the circumstances into account,) that the value of e will be reduced about one-half at the winter solstice. To find r at the same epoch, we have (p. 27) $6'{\cdot}24 : 1'{\cdot}4 :: r$, at equinox, $: r$, at winter solstice. The approximate values of r and e being thus found, eqs. (1), (2), (6), (7), (13), (14), (17), (19), give the values of R and E at the equinoxes and solstices, as well as of S and D. It will be seen on examining the Table that there is a pretty close correspondence between the determinations of S and D, except in the case in which $m=o$, that is in which the radial currents in existence at noon are supposed to continue in undiminished action during the afternoon. Owing however, to the small quantities neglected in obtaining the formulæ, and to the probability that the assumptions made are only approximately true, we cannot undertake to decide which, among the different suppositions made with regard to the values of m and n, corresponds most nearly to fact. We can only infer it to be highly probable that the radial currents in action at noon pass off almost entirely during the afternoon. Since the value of e is taken the same at each solstice, in eq. (17) $n(e'-e')=0$; the values of S and D given by eqs. (17) and (19) are therefore independent of n, and we can draw no inference with regard to the passing off of the ecliptic currents.

What is particularly important here to remark is that the close correspondence between the determinations of S and D serves as an additional verification of the explanation that has been given of the diurnal and annual variations of declination. We have already obtained a verification of our theory in the fact that the calculated semidiurnal effect of the ecliptic currents at the equinoxes is very nearly equal to half the excess of the midday declination at the autumnal equinox over that at the vernal equinox (p. 207). No account was taken of the residual currents, and we now see (p. 27) that the value of E comes out the same, at the equinoxes, whether these currents be taken into account or disregarded. The value of E for the forenoon is $1'{\cdot}49$; $2\times 1'{\cdot}49 = 2'{\cdot}98$. The average values, for forenoon and afternoon, (whether we use Table I or II,) is $2'{\cdot}02$, $2\times 2'{\cdot}02 = 4'{\cdot}04$. The actual deflection is $3'{\cdot}23$.

In the explanation of the deflection of the needle (at 6 A. M.) in the interval between the solstices it was implied that each of the two systems of currents had the same tendency to deflect the needle toward the west at the winter solstice, that it had to deflect it toward the east at the summer solstice ;—that is that the morning currents had the same intensity at these two periods. This idea is however by no means essential to the explanation. It does not accord with fact, for the deflection in the interval between the equinoxes and either solstice would then be half of the whole deflection ; whereas the deflection from the equinoxes to the summer solstice is 3′·47, and from the equinoxes to the winter solstice 1′·43. Both sets of currents are concerned in producing this inequality. As for the morning radial currents they proceed from all the points of the photosphere which are exposed to the sun, but the character of the result may be ascertained by considering the more effective currents. These proceed, at the summer solstice, from the points which lie to the north of the prime vertical ; and at the winter solstice from the points that lie to the south of the prime vertical. The former system of points are nearer to the zenith than the latter, and hence the deflection, so far as it depends on the radial currents, should be greater at the summer than at the winter solstice. It will be seen on referring to the Table on p. 29 that the effect of the ecliptic currents is also greater at the summer than at the winter solstice. The values of E at these two periods are about in the ratio of 4 to 1. The inequality in these two values seems to imply that the excess of the morning currents of the northern hemisphere over those of the southern hemisphere, at the summer solstice, is greater than the excess of the morning currents of the southern hemisphere over those of the northern at the winter solstice. In accordance with this conclusion we have the fact already alluded to that the individual currents of the northern hemisphere (or at least a portion of them) have a much higher intensity in the winter than in the summer. But it remains to be ascertained, by a discussion of the variations of declination observed at some station in the southern hemisphere, whether the currents of the southern hemisphere do not experience a corresponding diminution at the southern solstice.

HORIZONTAL FORCE.

Diurnal Variations.—The curve of the diurnal variations of the horizontal force has two maxima and two minima : the maxima occur about 4 A. M. and 4 P. M., and the minima about 10 A. M and 10 P. M. It may be conceived, for the sake of more ready comprehension, as representing a flood and ebb tide in the ethereal sea of magnetic force ;—the full flood tide lagging behind the sun about 4 hours, at noon and midnight, and the ebb

tide following the flood at an interval of about 6 hours. But it is to be observed that the fall and rise during the night are comparatively slight, and that in the summer months the nocturnal rise sometimes is altogether wanting;—the decrease continuing from 4 P. M. to 10 A. M. The principal minimum is at 10 A. M. to 12 M.; the principal maximum is at 4 P. M. in the summer, and generally at 4 to 6 A. M. in the winter. These phenomena may be referred to the combined action of the two systems of electric currents, radial and ecliptic, by which the corresponding variations of the declination have been explained. In the present case we have the effects of the components of these currents, which cross the meridian at right angles, but in the case of the variations of declination, the effects of the components running in the direction of the meridian. To consider first the effects of the perpendicular components of the radial currents; let us confine our attention for the moment, to the single current proceeding from the point immediately underneath the sun. This is represented by P*h* in Fig. 2 (p. 189); S being supposed to be the point of the photosphere which has the sun in its zenith. During the whole of the forenoon it will be directed toward the west and the horizontal force will be diminished, and during the whole of the afternoon it will be directed toward the east and the horizontal force will be augmented. If we suppose the intensity of the current arriving at P from the point S to be the same for all positions of S on the parallel of latitude AB, P*h* will have a maximum value when the sun is on the prime vertical at A, and again when it is on the same circle, at B, in the afternoon. During the forenoon it will decrease, and vanish altogether at noon. In the afternoon it will go through the same changes in the inverse order. The minimum of horizontal force should therefore occur early in the forenoon and the maximum late in the afternoon.

But in point of fact while the sun is moving from A to M the distance SP is diminishing, and therefore the current propagated from S increasing. The component P*h* is also proportional to cos APS, and therefore at first varies very slowly. It may very well happen therefore that the minimum will occur sometime after the sun is at A, and the maximum sometime before he is at B. Again, instead of a single current proceeding from S, there is actually, according to our theory, a similar current emanating from all the points of the photosphere which are exposed to the suns's action, and the minimum of horizontal force obtains when the resultant of all the actions of these innumerable currents, directed toward the west, begins to decrease. At what hour this will occur cannot be determined, until we have ascertained the law of variation of the currents traversing arcs of different lengths. We can only say that it doubtless will occur before the middle of the forenoon; and the maximum of horizontal force, about the

same hour in the afternoon. The tendency of residual currents, so far as they may act, will be to make the minimum happen later in the forenoon, and the maximum later in the afternoon.

But the ecliptic currents play a no less important part than the radial, which we must proceed to consider. At noon, or near that hour, these currents will be perpendicular to the meridian, but about 6 A. M. and 6 P. M. their obliquity to the meridian will be at its maximum. These currents will always tend to diminish the horizontal force, and will act with the greatest effect when their obliquity is the least; since their components in the direction perpendicular to the meridian will then be the greatest, and for the additional reason that the currents themselves are then the most intense. Their tendency therefore is to make the minimum of horizontal force fall at or near the hour of noon, and the maximum at about 6 A. M. and 6 P. M. Acting as they do, in conjunction with the radial currents, they will have the effect to make the minimum fall later in the forenoon, and the subsequent maximum later in the afternoon. The relative effect of the two sets of currents, it is reasonable to suppose, should however be different in different seasons, and hence the hours of maxima and minima vary somewhat, as they are known to do, from one season to another.

To ascertain the comparative semidiurnal effects of the ecliptic and radial currents, denote them respectively, by R and E, and let m = variation of horizontal force from 6 A. M. to noon, and m' = variation from noon to 6 P. M; then $m=\mathrm{R}-\mathrm{E}$, $m'=\mathrm{R}+\mathrm{E}$; whence $\mathrm{E}=\frac{m'-m}{2}$, and $\mathrm{R}=\frac{m'+m}{2}$...(20). It is to be observed that R is equal to the effect of the component, perpendicular to the meridian, of the resultant of the radial currents at 6 A. M., and nearly equal to that of this resultant itself; and that E is nearly equal to the effect of the ecliptic currents at noon. We here leave out of view the residual currents. To take them into account let r = effect on horizontal force of residual radial currents at noon; r' = same at 6 P. M.; e = effect of residual ecliptic currents at noon; e' = same at 6 P. M.; k and l = effects of nocturnal radial and ecliptic currents respectively, in action at 6 A. M. (or rather of that portion of them which passes off during the forenoon); pr = the proportion of residual and qe = the proportion of ecliptic currents that pass off during the afternoon, then

$$m=\mathrm{R}-\mathrm{E}-r-e+k+l,\ m'=\mathrm{R}+\mathrm{E}+r'+pr+qe-e' \quad \ldots \quad (21),$$

$r=r'$ (nearly) and $e=e'$ (nearly); k and l are comparatively small; the values of p and q lie between o and 1, but are probably greater than $\frac{1}{2}$. We have then,

$$\mathrm{E}=\frac{m'-m}{2}-\frac{r(2+p)+qe}{2}\ \text{(nearly)},\ \mathrm{R}=\frac{m'+m}{2}-\frac{pr+e'q-2)}{2}$$

(nearly) . . . (22). From which it appears that both E and R are less than as determined by eqs. (20), and that the diminution of E, by reason of the residual currents, exceeds that of R by the quantity $r+e$.

The nocturnal variations of the horizontal force, like the diurnal, are the joint effect of the two systems of currents; and the tendencies of each system are the same as during the day. The intensity of the currents is only diminished; that of the radial currents because these currents now proceed from the portion of the photosphere which is diametrically opposite to the sun (p. 191) and are weakened by the currents proceeding from the points underneath the sun; and that of the ecliptic currents because they come from the southern hemisphere, and at midnight have traversed an arc of 180°. In the winter of northern latitudes the radial currents which proceed from the part of the photosphere diametrically opposite to the sun, come from points in the northern hemisphere, and hence the nocturnal rise of horizontal force should be greatest in the winter. In the summer the corresponding currents, proceeding from points in the southern hemisphere, in conjunction with the nocturnal ecliptic currents, generally occasion a slight augmentation of horizontal force after midnight; but in some instances they only suffice to check the tendency to a diminution, from the action of the radial currents propagated from the points underneath the sun, and from the gradual subsidence of the residual currents consequent upon the sun's action during the day. (See curves of variations of horizontal force during the summer months, at Toronto and Hobarton, in the Hobarton Observations, Vol. I, p. 54.)

Annual Variations.

These as observed at Toronto, may be studied in Table III, on the opposite page.

The principal annual variations are: 1, An inequality in the diurnal range, the amount of the range being considerably greater from April to September than from October to March: 2, A change in the hours of the maxima and minima; especially in the hour of the principal minimum, which is at 9 or 10 A. M. from April to October inclusive, and at 11 A. M. or 12 M. from November to March inclusive: 3, A variation in the mean monthly value of the horizontal force; the maximum being in June and the minimum in December. Preparatory to the explanation of these variations let us compare the mean semidiurnal effects of the radial and ecliptic currents in the different months of the year. For this we have (disregarding the residual currents) the eqns. (20) (p. 33), which give the following results:

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
R	10·5	18	29·5	53·5	56	58·5	50	48	48	23	3	29
E	108·5	82	97·5	78·5	39	48·5	36	43	99	72	104	96

These values of E show approximately, the comparative mean intensities of the ecliptic currents at noon, in the several months, and the values of R the intensities of the radial currents at 6 A. M. or 6 P. M. These numbers also represent the relative semidiurnal effects of the two systems of currents. It appears that the intensity of the radial currents is twice as great in June as in December, while the intensity of the ecliptic currents is twice as great in December as in June. It would naturally be supposed that the radial currents would be more intense in summer than in winter; and it is to this fact that we are to attribute the increase in the diurnal range of the horizontal force from winter to summer. If we take the range from 6 A. M. to 6 P. M. it will be due entirely to the action of the radial currents, and will be proportional to R. The range from the morning minimum to the afternoon maximum should be greater than 2R, at the summer solstice, because the greatest action of the radial currents to diminish the horizontal force obtains a certain length of time after the passage across the prime vertical (p. 32), and the greatest action to augment the horizontal force before the passage across the prime vertical in the afternoon.

The annual variation of the hour of the principal minimum is another consequence of the change that occurs during the year

TABLE III.

Mean Diurnal Variation of the Horizontal Force in each Month of the Year, at Toronto; derived from Hourly Observations in the Years 1843 *to* 1848.

Astron. time at Toronto.	Jan.	Feb.	Mar.	Apr	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Apr. to Sept. inc.	Oct. to Mar. inc.	Ann. Means
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
0h	003	008	008	026	057	054	055	053	068	039	014	000	052	010	026
1	036	039	043	060	100	091	074	098	149	068	040	022	095	039	062
2	069	069	084	099	144	138	126	141	205	110	084	067	142	079	105
3	108	107	129	143	169	168	148	171	245	134	112	096	174	113	138
4	141	126	147	164	181	178	161	175	253	155	128	127	185	135	155
5	136	130	149	173	174	180	158	173	238	151	120	126	183	133	153
6	122	108	135	158	152	161	141	144	215	134	121	125	162	122	137
7	117	107	124	129	144	139	130	122	190	122	120	124	142	117	125
8	112	102	118	108	120	112	106	115	178	111	113	108	123	109	111
9	101	087	103	096	116	092	095	103	171	102	104	097	112	097	100
10	099	085	088	090	089	082	078	091	164	097	086	086	099	088	089
11	090	080	074	053	081	068	066	069	151	085	085	084	081	081	076
12	069	064	071	060	064	062	051	068	139	083	075	060	074	068	066
13	066	046	060	047	052	052	048	051	123	061	072	057	062	058	055
14	057	042	064	005	041	051	041	052	115	047	079	069	051	058	049
15	071	049	063	020	029	041	030	039	111	073	084	072	045	067	051
16	085	052	070	051	045	041	027	045	134	061	091	068	057	069	058
17	091	065	069	059	031	044	030	045	131	091	102	054	057	077	062
18	101	072	076	051	040	044	041	048	119	088	115	067	057	085	066
19	103	036	060	038	042	039	044	036	091	058	103	078	048	071	055
20	088	026	038	029	028	030	033	023	064	024	070	067	035	050	037
21	060	023	015	015	002	014	016	001	020	000	030	045	011	027	014
22	024	008	005	000	000	000	000	000	000	003	011	022	000	010	000
23	000	000	000	001	020	017	024	016	017	004	000	006	016	000	003

in the relative intensities of the two systems of currents. In the summer the radial currents are more efficient than the ecliptic, but in the winter the ecliptic currents are much more efficient than the radial; and the tendency of the radial currents is to produce a minimum early in the forenoon, and of the ecliptic currents to produce a minimum at noon. It follows therefore that the actual minimum should occur later in the forenoon in the winter than in the summer.

The increase in the mean monthly value of the horizontal force from the winter to the summer solstice, finds its explanation in the varying action of the ecliptic currents. In the course of a single day the diminution and augmentation of the horizontal force that result from the action of the radial currents balance each other, and hence the variations in the mean daily values of this force from one month to another, must be ascribed to the annual variation of the ecliptic currents. Now these currents have the highest intensity in the winter (p. 35), and always tend to diminish the horizontal force; therefore the mean daily value of this force should be less in winter than in summer. The following numerical results furnish a strong confirmation of the truth of this explanation. Mean annual intensity of the ecliptic currents, at Toronto, for the years 1843–8, (or mean of the values of E in Table on p. 34)=75·3; mean intensity of same for the months of Jan., Feb., March, Sept., Oct., Nov., and Dec.= 94·1; mean intensity for April, May, June, July, and Aug.= 49·0. From which 94·1−75·3=18·8; 75·3−49·0=26·3. Again mean value of horizontal force, at Toronto, from 1845 to 1851, (allowing for secular change, Toronto Observations, vol. ii, p. 91)=3·53088; mean horizontal force for the months of Jan., Feb., March, Sept., Oct., Nov., and Dec. =3·52973; mean horizontal force for April, May, June, July, and Aug. =3·53250. Whence 3.53088−3·52973= ·00115; 3·53250−3·53088=·00162. Now ·00162÷·00115= 1·409; and 26·3÷18·8=1·400. From Sept. to March, inclusive, the ecliptic currents are the most energetic, and have pretty nearly the same intensity. The intensity in April is as high as the lowest intensity during this interval. If we include April in the months of highest intensity, that is compare January, February, March, April, September, October, November, December, with May, June, July, August, we have a still closer agreement of results, for the quotients come out respectively 2·00, and 1·99.

VERTICAL FORCE.

Diurnal and Annual Variations.—It is first to be observed that, agreeably to the general theory which is under consideration, the diurnal and annual variations of the vertical force are to be referred entirely to variations in the intensity of the ecliptic currents. The radial currents which diverge from any point of the

photosphere, and pass near the zenith on opposite sides, will neutralize each other in their action on the vertical force, since they will be of equal intensity. But the ecliptic currents that lie on opposite sides of the zenith will in general be of unequal intensity, and will affect the vertical force in proportion to the amount of their difference of intensity. Any variations that may take place in this difference will accordingly be attended with variations in the intensity of the vertical force. If we consider the entire set of ecliptic currents above the horizon it will be seen that the perturbations of the vertical force will be chiefly produced by the currents which are remote from the zenith; for the proper tangential force of any single current will lie in the plane of the horizon if the current passes through the zenith, but will lie in the vertical line if the current is in the horizon, and will then be wholly effective in disturbing the vertical force. The currents which are below the horizon have the same tendency as those which are above it, but they have less effect. In the case of the horizontal force the currents above and below the horizon have opposite tendencies. If it should seem improbable that the differences of intensity of the ecliptic currents should be the occasion of sensible disturbances of the vertical force, when the direct effects of these currents on the horizontal force are slight, it is to be observed, in addition to the distinction just noticed, that the effect on the vertical force of two currents on opposite sides of the zenith, at the distance of 30°, is one half of the entire force due to the difference of the currents; also that the surface of the atmosphere crosses the plane of the horizon under a small angle, and hence that the portion of the photosphere which is in most effective action is very large in comparison with that in the vicinity of the zenith which is principally concerned in modifying the horizontal force. If we suppose the height of the atmosphere to be about 60 miles 61 (miles) its surface will cross the plane of the horizon under an angle of 10°, and the length of an arc extending 45° from the zenith will be about 60 miles while the length of its continuation down to the horizon will be about 640 miles.

In studying the effects of the ecliptic currents on the vertical force we must observe that any current which passes to the south of the zenith tends to augment the vertical force, and any current which passes to the north to diminish it; again that any current which, coming from the north, passes to the east of the zenith tends to augment the vertical force, and any current which, coming from the same quarter, passes to the west of the zenith tends to diminish it; also that in the case of currents coming from the south the effects are just reversed. The variations experienced by the vertical force on any particular day must depend on the law of variation of the ecliptic currents along the circle of excitement (p. 200), on the

law of variation of the individual currents with the distance from the points at which they originate, and on the position of the diurnal circle traced by the zenith among the ecliptic currents, or on the sun's distance from the zenith at noon. The previous discussion has not revealed these laws, it has only served to confirm the natural supposition that the intensity of each individual current decreases in both directions from the point of original excitement, and made known the comparative intensities of the currents which pass through the zenith at noon in the different months of the year. Avoiding, for the present, all speculation as to the mode of excitation of the ecliptic currents, the most probable supposition that can be made with regard to the relative intensities of the different currents developed at any instant, is that at any particular instant the current which is at any given distance from the point underneath the sun has the same intensity as the current which passes through the zenith on that day on which the zenith is at the same given distance from the point under the sun. This is equivalent to supposing that the intensity of a current depends in every instance, entirely on the distance of the point at which it originates from the point of the photosphere which has the sun in its zenith. It would seem that this should be the case unless there are inequalities in the photosphere. Adopting this supposition, then, we may infer from the results given in the Table on p. 34 that, beginning at a point about 20° from the sun, the ecliptic currents gradually increase in intensity until we reach the distance of about 45°, but that from this point to 67° they retain pretty nearly the same intensity. Beyond 67°, toward the horizon, we must conclude that the currents in question, if they have their origin in any direct solar action, in all probability diminish in intensity. Whatever may be the law of distribution of the ecliptic currents over the photosphere, at any moment, it is important to remark that, except as there may be inequalities in the photosphere, it should be the same at all seasons; and therefore that the laws of the diurnal variations of the vertical force should be the same at corresponding seasons in both hemispheres (*e. g.* at Hobarton as at Toronto).

If the above conclusions are correct then the difference of intensity of currents on opposite sides of the zenith must be greater at 6 A. M. or 6 P. M. than at noon, and hence there should be a tendency to a diminution of the vertical force from 6 A. M. to noon, and to an augmentation from noon to 6 P. M. But if the ecliptic currents after their excitation subside gradually, from hour to hour, we shall have in action at noon the differences of all the residual currents consequent upon the forenoon excitation, and at 6 P. M. the differences of all the residual currents resulting from the sun's action during the whole day. The tendency of these residual currents will be to make the vertical force greater

at noon than at 6 A. M., and greater at 6 P. M. than at noon, unless the currents in action at noon should pass off entirely during the afternoon. The effect of the residual currents at noon being approximately due to the sum of a series of successive differences, were it not that these differences have decreased according to some unknown law, would be approximately proportional to the difference between the ecliptic currents at 6 A. M. and at noon. In the play of the two tendencies just mentioned I conceive that we have the general explanation of the diurnal variations of the vertical force.

TABLE IV.

Mean Diurnal Variation of the Vertical Force in each Month of the Year, at Toronto; derived from Hourly Observations in the Years 1843 *to* 1848.

Astron. time at Toronto.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Apr. to Sept. inc.	Oct. to Mar. inc.	Ann. Means
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
0h	007	008	006	015	011	002	015	028	036	025	013	016	017	013	015
1	011	011	011	024	017	004	018	033	046	031	018	022	023	017	019
2	515	017	017	028	026	011	027	040	052	035	026	030	030	023	025
3	019	019	022	033	032	017	034	046	056	037	027	030	035	026	029
4	016	022	023	037	037	021	040	052	056	036	031	030	040	026	031
5	019	023	025	038	042	023	044	052	051	039	029	028	041	027	032
6	018	022	026	037	039	023	041	048	050	038	027	029	039	027	031
7	019	023	028	034	038	022	038	044	048	040	031	030	036	028	031
8	019	023	025	030	033	018	032	039	044	038	028	028	032	027	028
9	017	018	023	028	029	015	023	030	038	032	021	028	031	023	023
10	016	017	015	021	021	011	021	022	031	029	016	025	020	021	018
11	012	014	007	008	016	008	014	009	029	024	009	020	013	014	012
12	008	007	002	004	007	004	007	005	019	014	008	010	007	008	006
13	007	001	000	003	008	000	001	001	007	009	006	010	002	006	002
14	002	000	000	000	000	001	000	003	000	009	002	011	000	004	000
15	001	001	000	001	003	000	003	000	001	005	003	013	001	004	001
16	001	000	000	003	011	006	007	007	004	000	000	001	005	000	001
17	001	002	001	006	014	011	014	019	009	005	002	000	011	002	005
18	004	004	007	014	020	014	017	028	017	008	005	005	017	005	010
19	005	004	010	011	017	013	018	029	022	020	007	003	017	008	011
20	007	015	011	012	017	011	018	027	020	026	009	009	016	013	013
21	001	005	006	012	013	007	017	024	021	019	006	009	015	008	010
22	000	002	003	011	009	004	014	024	024	020	007	012	013	007	009
23	003	002	002	011	009	001	014	024	028	019	009	013	014	008	009

"The diurnal variation of the vertical force at Toronto in both seasons, *i. e.*, from April to September inclusive, and from October to March inclusive, is a double progression, having two maxima and two minima. The principal maximum takes place two hours earlier from April to September than from October to March, viz, at 5^h from April to September, and at 7^h from October to March. From this maximum the diminution is progressive to the principal minimum, which also occurs earlier from April to September than from October to March; *i e.*, between 14^h and 15^h (2 A. M. and 3 A. M.) from April to September; and at 16^h (4 A. M.) from October to March. The secondary minimum is at 22^h (10 A. M.) in both seasons. The range of the diurnal variation is greater

during the six months when the sun is north of the equator, or from April to September, than in the opposite season."

We have seen (p. 38) that there are two causes in operation tending to produce a diurnal variation of the vertical force; in the forenoon they are opposed to each other but in the afternoon conspire to augment the vertical force. It would seem that at Toronto the tendency to an increase, by reason of the residual currents, prevails early in the forenoon over the tendency to a diminution, resulting from the decrease in the difference of intensity of the currents on opposite sides of the zenith. Later in the forenoon the other tendency prevails and a secondary minimum results. The range from 6 A. M. to 6 P. M. is entirely due to the residual currents.

Let c= effect on the vertical force of the difference of intensity of the ecliptic currents on opposite sides of the zenith, at noon; c'= same at 6 A. M. or 6 P. M.; s= effect of sum of residual currents at noon, resulting from forenoon excitation, or of residual currents at 6 P. M. resulting from afternoon excitation, m= rise of vertical force in the interval from 6 A. M. to noon; and m'= same from noon to 6 P. M.; then, supposing the residual currents in action at noon to continue without diminution until 6 P. M.,

$$s-(c'-c)=m,\ s+(c'-c)=m';\ s=\frac{m+m'}{2},\ c'-c=\frac{m'-m}{2}\ldots(23).$$

Or supposing the residual currents at noon to pass off entirely during the afternoon,

$$s-(c'-c)=m,\ c'-c=m';\ s=m+(c'-c)\ \ldots\ (24).$$

The following Table contains the values of s and $(c'-c)$ for each month in the year, computed from equations (23) and (24), and the means of these values.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
s	7	9	9·5	11·5	9·5	4·5	12	10	16·5	15	11	12
s	14	18	19	23	19	9	24	20	33	30	22	24
Means,	10·5	13·5	14·25	17·25	14·25	6·75	18	15	24·75	22·5	16·5	18
$c'-c$	4	5	10·5	10·5	18·5	16·5	14	10	−2·5	−2	3	1
$c'-c$	11	14	20	22	28	21	26	20	14	13	14	13
Means,	7·5	9·5	15·25	16·25	23·25	18·75	20	15	5·75	5·5	8·5	7

The means, which answer approximately to the supposition that the residual currents in action at noon are reduced in intensity one half at 6 P. M., probably correspond most nearly to fact.

If we inquire into the phenomena of the variations of the vertical force at other stations, we find that there is a considerable diversity. For example at Philadelphia, the maximum is generally at 2 P. M., and the force decreases from this hour to about midnight, then increases until 2 A. M., and decreases again until 4 to 7 A. M.; from the hour of the morning minimum to that of the afternoon maximum it steadily increases, but the increase is much less rapid toward 10 A. M., especially in the winter, than before or after that hour. At Toronto this falling off in the augmentation amounts to the production of a secondary minimum.

"At Hobarton there is one decided minimum throughout the year, and in the winter of the southern hemisphere but one maximum. The minimum is at 7 A. M. in the summer, and at 9 A. M. in the winter. The winter maximum falls at 3 P. M. In the summer there are two maxima, at noon and at 6 P. M., the latter maximum being rather superior. Intermediate between these two afternoon maxima, is a small secondary minimum. The range of the diurnal variation is nearly the same at both periods of the year, and amounts to about four parts in a thousand of the whole vertical force." Both at Toronto and Philadelphia the amount of the diurnal range is greater in the summer than in the winter. This diversity in the phenomena at different stations indicates the existence of inequalities in different parts of the earth's photosphere, by reason of which the law of variation of the intensity of the system of primary ecliptic currents excited at a given hour by the sun is modified at particular localities, or what is more probable, the currents which have been developed subside more rapidly at some stations than at others. Thus we may explain the phenomena at Philadelphia by supposing that the residual currents pass off very rapidly in the afternoon but are particularly effective in the forenoon. The residual currents having passed off, the nocturnal currents come into sensible action, producing a secondary maximum at 2 A. M.; but at Toronto and Hobarton these currents serve only to modify the diminution of the vertical force, resulting from the slow subsidence of all the currents which are in operation at sunset.

IRREGULAR VARIATIONS.

While the periodical variations of the magnetic elements may be traced to periodical changes in the intensity and direction of propagation, of two systems of electric currents, radial and ecliptic, developed by some action of the sun in the upper atmosphere, the irregular variations so-called may be referred with a high degree of probability, to the occasional excitation of the same two systems of currents. The following are among the more prominent indications of the irregular action of the radial and ecliptic currents.

1. It appears from Colonel Sabine's discussion of the irregular variations as observed at Toronto, that the general effect on the horizontal force is a diminution, and the ecliptic currents must always have this effect; 2. The westerly variations of declination have a maximum both in number and value, at Toronto, about 7 or 8 A. M., and a minimum about 9 P. M., while the easterly variations have a maximum about 9 P. M., and a minimum about 3 P. M. These facts may be explained by the occasional action of radial currents, if we assume that the greatest liability to their excitation is always on the meridian which is from 4^h to 5^h to the west of the sun, and to the south of the latitude of

Toronto, for on this assumption the radial currents will, at 7 or 8 A. M., lie in the meridian, and proceeding from the south, will deflect the needle farther to the west than at any other hour, when the supposed meridian of maximum excitation will deviate more or less from the meridian of the station. Again on the same supposition, the meridian in question, preceding the sun some four hours, the currents originating on it and propagated to the zenith of the station will again lie in the meridian of the station at about 8 P. M., but they will now come from the north and deflect the needle toward the east. At the hour of the minimum of easterly disturbance, 3 P. M., when the westerly disturbance is also reduced to nearly its minimum of frequency and value, the points of especial excitation are in the vicinity of the prime vertical, and it is only the feebler currents proceeding from points remote from the meridian of maximum disturbance that will be inclined under a small angle to the meridian of the station; 3. In striking confirmation of the explanation just given, the hours of maximum disturbance of the horizontal force, in excess and defect, are removed about 6 hours from the hours of maximum disturbance in declination. Thus the disturbances augmenting the force are most numerous from noon to 4 P. M., when the points of most frequent excitation are near the prime vertical toward the west, and their radial currents are in the position to have the greatest effect in augmenting the horizontal force. Again the disturbances in defect predominate especially at from 10 P. M., to 4 A. M., when the points of most frequent disturbance are near the prime vertical toward the east, and when the current propagated from them to the zenith should have the greatest effect in weakening the force. Also the minimum of disturbance in excess is at the hour of the maximum of disturbance in defect, and the minimum of disturbance in defect is at the hour of the maximum disturbance in excess. The disturbances in defect preponderate over those in excess at all hours; in this fact, as already intimated, we have an evidence of the operation of occasional ecliptic currents. These should have a high intensity in the afternoon, when the disturbances in excess are the most frequent, and may accordingly cause the disturbances in defect to preponderate then. The preponderance is but slight (1·3 to 1 for the years 1841–2). 4. The westerly disturbances of declination decline in numbers and value for about 12 hours before and after the maximum, with but one or two slight irregularities; and the same is true of the easterly disturbances (except that there is slight increase from 6 hours to 12 hours before the hour of maximum). This fact indicates that the points of the photosphere at which the irregular excitation occurs extend some 6 hours from the meridian of maximum excitement in both directions. Other indications might be pointed out but these will suffice for our present purpose.

As to the exciting cause of these irregular currents, the occasional ecliptic currents can only be referred, it would seem, to paroxysmal disturbances of the sun's magnetic force; which disturbances, from what we have already seen (p. 185), must be in some way connected with those mighty processes going on at the sun's surface that reveal their action by giving rise to the appearance of dark spots on the disk. The irregular radial currents cannot be ascribed to the sudden augmentation of some impulsive force emanating from the sun, for such a force would have its greatest effect on the meridian over which the sun was, and thus the maximum of westerly disturbance of declination would be at noon, instead of 7 or 8 A. M. They may be supposed to have their origin in occasional electric discharges occurring between the upper and lower atmosphere, attended with the flow of horizontal currents from or toward the points of discharge, but the difficulty still remains, of assigning any plausible reason why the meridian of greatest excitement should continually precede the sun 4 or 5 hours in its diurnal course, and the disturbance of the electric equilibrium decrease in both directions from this meridian. We can only conjecture that this fact might possibly be attributed to variations of atmospheric humidity. But there is another and more plausible supposition that may be made; it is that the irregular radial currents have their origin in the sudden arrival at certain points of the upper atmosphere, from time to time, of some material emanation radiating from the sun's surface; and either by its force of impact or by an electric action, developing electric currents diverging in every direction over the photosphere. If we conceive the supposed material emanation from the sun to have, when it reaches the earth, the same velocity as the earth in its orbit, then, confining our attention for the moment to the circle of intersection of the plane of the ecliptic with the surface of the photosphere, the radiant matter would continually arrive in the direction normal to the surface at the point of this circle which is 45° to the west of the sun. If now we consider portions of the photosphere to the north or south of the ecliptic, it will be seen on a little reflection that the direction of approach will deviate the least from the normal at the points which are situated on the great circle traced through the point of the ecliptic 45° to the west of the sun and perpendicular to the ecliptic. From this circle, in both directions parallel to the ecliptic, the obliquity of the solar emanations to the surface of the photosphere will gradually increase. If now we assume the velocity of these material radiations to be less than that of the earth in its orbit, the circle just mentioned will be farther to the west; the limit of distance from the sun being 90°, answering to the supposition that the radiant velocity is zero. To satisfy the results of observation this circle must be conceived to be from 60° to 75° to the west of the sun. We shall have a simple general

conception of the entire action of the solar emanations if we observe that the effect is the same that it would be if the sun were constantly from 60° to 75° to the west of its actual position, and the earth had no orbitual motion.

This view of the origin of the irregular perturbations of the directive force of the magnetic needle connects theoretically these phenomena with the appearance and disappearance of the solar spots; since it is natural to suppose that the material radiations from the sun are connected with those disturbances at the surface of the sun which are attended with visible effects. It may be conjectured that these perturbations may originate in electric disturbances in the upper atmosphere attendant upon the earth's encounter with the meteors that lie in its path; but this view appears to be irreconcilable with the fact that the circle of greatest excitement is only 60° to 75° to the west of the sun, instead of 90°. Besides it furnishes no link of connection between the perturbations and the sun's spots.

I have shown, in previous publications, that in the idea of material emanations from the sun's surface, according to a certain law, we have an adequate explanation of the Zodiacal Light; also that the matter thus radiated from the sun is probably the substance of terrestrial Auroras; and that it plays an important part in Cometary phenomena.

Note.—The same effects which in the present discussion have been referred to electric currents developed in the upper atmosphere, would be produced by currents circulating at the earth's surface in the opposite direction; that is, the one set converging toward the points of the surface which are exposed to the sun, and the other running from west to east, and at the outset parallel to the ecliptic. But there are several objections to the adoption of this view as a physical fact: *e. g.*, now that it has been established that evaporation does not disturb the electric equilibrium at the surface of the earth, there is no known action which can give rise to currents of positive electricity converging to the points on which the sun acts, and the tendency of any supposed electric force propagated from the sun would be to create diverging instead of converging currents. Again if the sun has the tendency to develop in the crust of the earth currents running from west to east, then the sun cannot have been the exciting cause of the normal magnetism of the earth, since the permanent currents run from east to west. Other objections might be stated.

The fundamental conception of two systems of electric currents which has served for the explanation of the disturbances of the magnetic needle, when transferred to the contiguous molecules of bodies conceived to be surrounded with electric atmospheres, appears to afford a sufficient basis for a satisfactory *dynamical* theory of frictional and galvanic electricity, and of the principle of magnetism in its various aspects.

Art. IV.—*On the Periodical Rise and Fall of the Lakes;* by Major Lachlan.*

(Concluded from vol. xix, p. 175.)

Leaving any further remarks on the foregoing for a future probable opportunity, I may here briefly observe, that I have long been persuaded that the severity of our winters is mitigated by the proximity of the Lakes, and is not so much owing to the prevalence of winds from the northwest, as a mere northerly point of the compass, or to the remarkable curve of the great isothermal line in this part of the globe, as to the winds alluded to sweeping down from a *more elevated region*, many parts of the extensive mountainous tract of country stretching in that direction being perhaps thousands of feet above the level of Lake Superior, and even the latter not being less than 600 feet above that of the ocean.

Nearly the whole of the conflicting evidence bearing on the various points at issue having been adduced, I proceed to state freely, yet as briefly as possible, the mode of proceeding adopted by me, in my endeavor to arrive at the convictions to which I have been thereby led with respect to each of the three questions to be determined.

To commence with the first of these, namely, the traditional report of there being a septennial rise and fall in the waters of the great Lakes, &c., I have to remark, that being unwilling to admit any assertions on so interesting and mysterious a phenomenon without thorough examination and comparison with facts, I, after much reflection, determined to attempt to form from the materials in my possession a general comparative tabular view of the positively known, and, failing that, generally acknowledged, periods of elevation and depression throughout the whole of the Lakes, during the longest ascertainable series of years; in the hope of thereby arriving at something like an approximation to the real state of the matter: but after laboring long and patiently at the unsatisfactory task, I was at last obliged to abandon it and confine my synopsis to Lake Erie alone, and even then to add various "Miscellaneous Remarks" for the mention of any apparent coincidence, or otherwise, in the state of the other Lakes; and in this I continued to persevere till, after much labor, I so far succeeded, as is shown in the following copious yet imperfect Table, exhibiting not only the various progressive and retrogressive annual fluctuations in the level of that particular Lake during a course of sixty-three years, as vouched by the different highly respectable authorities named, but also proving, incidentally, how far that long received traditional phenomenon, the rise and fall of the Lakes generally every seven years, is in accordance with the evidence furnished by recorded facts:—

* From the Canadian Journal, July, 1854.

Comparative view of the Rise and Fall of the Waters of Lake Erie, for sixty-three years in succession, as far as ascertained from the best sources of information within reach.

Date.	Comparative Level.	Authorities.
1790	1*st maximum*; being 5 ft. 6 in. above lowest level.	Prof. Hall, Higgins, Whittlesey, Mather, &c.
1791		
1792–94	No information regarding these years.	
1795, '96	1*st minimum*. Level described as low, but not exactly stated.	Weld, Whittlesey, &c.
1797–99	Rising, but amount not stated.	Do. do.
1800–2	High. 2*nd maximum*.	Higgins, Houghton, Whiting, &c.
1803–5	No information whatever.	
1806	Level low.	Whittlesey, &c.
1807–9	Rising. No information.	
1810, '11	2*nd minimum*. Reported as 6 feet below 1838.	Do.
1812–14	Waters rising.	Houghton, Higgins, Dearborn, Whiting, &c.
1815	3*d maximum*, but 2 ft. less than 1838.	Do. do. do.
1816	Same as last year.	
1817, 18	Falling.	Houghton, Higgins, &c.
1819, 20	3*d minimum*, or zero.	Houghton, Higgins, Whittlesey, and Whiting.
1821	Rising rapidly.	Do., do., do., Dearborn, M'Taggart.
1822	Do., but still low.	Do.
1823	Up to average or mean.	Do.
1824–26	Gradually rising, to within 2 feet of maximum of 1838.	
1827, '28	4*th maximum*, reckoned as high as in 1815.	Houghton, Higgins, Whiting, Dearborn, M'Taggart, &c.
1829	Still high.	
1830	As high as in 1828.	
1831	Subsiding rapidly.	Whiting, &c.
1832, '33	4*th minimum*, though only down to average.*	Whittlesey, Higgins.
1834	Rising.	
1835	2 feet 10 inches below 1838.	Houghton, Higgins, Mather, Whiting, Whittlesey, &c.
1836	1 foot 8 " " "	
1837	0 " 9 " " "	
1838	5*th maximum*. 5 feet 3 inches above zero.	Amer. Jour. Sci., Prof. Dewey, Buffalo Express.
1839	3 feet 8 inches above zero.	
1840	3 " 5 " " "	
1841	3 " 1 " " "	
1842	3 " 7 " " "	
1843	2 " 8 " " "	
1844	2 " 11 " " "	
1845	3 " 0 " " "	
1846	2 " 0 " 5*th minimum*.	
1847	2 " 6 " " zero.	
1848	2 " 2 " " "	
1849	3 " 1 " " "	
1850	2 " 8 " " "	
1851	2 " 11 " " "	
1852	Rising rapidly.	
1353	6*th maximum*; very high, as in 1832.	
1854		

N. B.—In contrast to Lake Erie, from 1846 to 1852, Lake Ontario was as follows at mouth of Genessee River :—

2 ft. 3 in. from top of dock.
1 " 1 " "
2 " 1 " "
1 " 9 " "
1 " 5 " "
1 " 11 " in July, "
1 " 2 " "

* By *average* is meant the *mean*, or half-way between the two extremes—say 2 feet 9 inches below the maximum of 1838.

Miscellaneous Remarks.—From 1788 to 1790, the Lakes generally, and Lake Erie in particular, stated to have been as high as in 1838, at which time, according to different authorities, compared with the lowest level known, it was estimated at 5 ft. 3 in., 5 ft. 4 in., and even 6 ft.; and Prof. Hall mentions evidence of a higher level than in 1838, in ridges and submerged trees.

During 1795 and 1796, *Lake Ontario* described as so high as to have drowned orchards near Kingston of 30 years growth, while the gravelly beach of Lake Erie near Cleveland was used as a road, and continued so for many years afterwards.

In 1798, Lake Erie reported as higher than in 1796.

Waters of Lake Erie, and of the others generally, high from 1800 to 1802; and the level loosely estimated as similar to that of 1827.

1806, level reported, in general terms, as low.

The level of 1810 is compared with the floods of 1790 and 1838: which would give about 2 ft. 9 in. below the mean level.

Gen. Dearborn states, from personal knowledge, that *Lake Erie* was, in 1814, more than 2 feet higher than in 1813, and that the river Detroit was unusually high during that and the following year, and much land submerged; Dr. Houghton describes the Detroit as high in 1814 and 1815; and Mr. Higgins the upper Lakes as full in 1814, and the central and lower Lakes in 1815.

In 1815, like previous year, Detroit and St. Clair Rivers unusualy full, and the rise of Ontario regarded as generally about 2 feet higher than the other Lakes.

In 1817 and 1819, an ebb and flow of from 14 to 18 inches, noticed at Green Bay by Major Storrow, and in 1820 by Mr. Schoolcraft, and in 1827 by Col. Whiting.

In 1819 and 1820, the central and lower Lakes described by Messrs. Higgins and Whittlesey as unusually low; while Col. Whiting and Dr. Houghton state that the Detroit River had resumed its usual level.

Lakes Huron and Erie described as having resumed their usual level during 1811.

In 1823 a rapid rise of 2 feet, from 5 to 3 feet below 1838.

In 1824, ice for a time blocked up outlet of Lake Huron, and River Detroit in consequence fell rapidly 10 feet; and a great depression took place in Lake Erie and Ontario, while the pent-up waters flowed back on Lake Huron. In other respects, the Lakes appear to have been in their usual state. In 1831, a similar occurrence.

In 1827 and 1828, Lake Ontario (and other Lakes) 2 feet higher than in 1820; and yet, according to Mr. M'Taggart (who estimates extra height between 2 and 3 feet), the rivers flowing out of them did not appear to be affected thereby; while Gen. Dearborn states that, though Lakes Erie and Ontario were so high, Lake Superior was lower than ever known before. In 1830, the level of Lake Erie rated at 2 feet above 1819.

In 1831, Lake Erie fell temporarily between 3 and 4 feet. (See also 1824.)

In 1835, Lake considered 1 ft. 8 in. higher than in 1819; and afterwards in 1842.

In 1836, level the same as in 1830, and subsequently in 1853, and 1 ft. higher than the previous year. *N. B.*—The figures in the "Comparative Level" column to 1838 from Mr. Higgins.

In 1838, Lake stated by Higgins to be 5 ft. 3 in. above 1819, and by Buffalo *Advertiser* 5 ft. 4 in. in June and 5 ft. 9 in. in August: and according to Dr. Houghton, it might be 6 ft.: much land overflowed and trees of 100 years growth killed. Lake Ontario said to be 6 ft. 10 in. above 1825, Lake Huron higher than for two centuries, Michigan 6 ft. higher than in 1820, and Superior said to be 3 ft. higher than usual, and 1 ft. above 1837. By February, 1839, Erie had fallen to 3 ft. 8 in. (see also 1827), and in 1840 it was higher than for 23 years before, with the exception of 1838. *N. B.*—The levels in figures from 1839 to 1851 from Buffalo *Advertiser* of April, 1851.

In 1844, all the Lakes considered low; but during the night of 18th October, Lake Erie suddenly rose temporarily at Buffalo 13 ft. 8 in. above the harbor zero, caused by a great storm. In 1845, a sudden rise and fall of Lake Ontario, caused (according to Prof. Dewey) by a tornado, with water-spout, thunder, and hail. In that year, however, Lakes Erie, Huron, and Michigan much lower than usual; and in Lake Superior a rock at the entrance of Eagle Harbor appeared above water, and next year was 1½ ft. high, and in next year still higher. In 1846, Gull Island (a lighthouse station in Lake Ontario) re-appeared, after having been submerged 7 years. In January, 1847, sudden flux and reflux of Lake Ontario near Cobourg, when the waters receded 350 ft. and returned in an unbroken wave 4 ft. high; repeated 7 or 8 times till it gradually assumed its usual appearance. On 30th March, outlet of

Lake Erie temporarily blocked up with ice, so as to leave the Table Rock at Niagara Falls, and 200 ft. beyond it, dry. On 18th April, a sudden temporary depression of Lake Erie at Buffalo to 22 in. below zero, caused by a strong gale from the N. E. In 1851, *Lake Erie* at Port Colborne, was 3 ft. higher than in 1850; and in 1852 very little change; and in 1853 level nearly the same as in 1838 and 1830. In 1852 *Lake Ontario* 1 ft. 2 in. higher than in 1851; and in 1853, 9 in. higher, and calculated to be the same as in 1830 and 1838, and 4 ft. 5 in. above the minimum of 1849. In 1853, the River St. Lawrence generally considered as very high.

General Remark.—It is estimated that the Lakes subside irregularly, between the great periodical floods, at the rate of about 1 ft. 4 in. per annum; but that the comparative rapidity of the *fall* is as about 2 years, to 5 of the rise; and that the waters remain for some time at the mean level. Mr. Murray observes of Lake Huron, in his Report of 1848, that its waters have sunk considerably below former (perhaps ancient) levels, as indicated by water-marks, to the extent of 4 feet 10 inches.

While leaving the details of the foregoing Table to speak for themselves, I may be permitted to superadd, that the column of "*Comparative level*," however imperfect, is as complete as my materials would furnish; and that the greater part of the names there registered will be found among the different writers to whom I have had occasion to refer, or quote from, in the course of the foregoing remarks, besides several other sources of information to which I may hereafter have to advert; and further, that I have, under the head of "*Miscellaneous remarks*," taken care to refer to all such information of a loose *comparative* nature as appeared too indefinite for being admitted into the column of "*Authorities*," though not altogether to be rejected as without value; as well as to note not only any remarkable temporary derangements in the usual flow of Lake Erie, but any coincidence of level, or other remarkable event, connected with the state of the other Lakes at the same time. From a careful perusal of all details, I am disposed to think it will, in the first place, be satisfactorily demonstrated that not only there is no *regular* septennial or other great flood in Lake Erie, or any other of the great Lakes, but that, though in 1838 the whole of our inland waters happened to be simultaneously at an extraordinary height, it is very problematical whether they will always be found in an elevated or depressed state at the same time. For instance, taking it for granted that 1789–90 was really a maximum year, it will be seen by a reference to the Table, that instead of an interval of 14 years, the next maximum took place in 10 years, or in 1800–1; and that the next great flood was fortuitously in 1815; but that the next was in 1828; the next in 1838; and the last in 1853; and that, as might be reasonably expected, the advent of the mean and maximum periods was still less to be depended on. I am therefore bound to coincide in the more rational, and now generally received *opinion*, that the intervals at which these extraordinary floods occur are, at the best, uncertain, and mainly dependent on the extra amount of rain and snow, and the less degree of evaporation during the summer months, in that par-

ticular year; and that though the rise and fall in the different Lakes may, under ordinary circumstances, be generally simultaneous, it does not follow that such will always be the case; or in other words, that there may sometimes be a rise for a season, or part of a season, in one Lake, altogether independent of the others, arising from temporary obstructions at its outlet—a conclusion which I have arrived at, after much inquiry, observation, and reflection, in addition to the evidence furnished in the foregoing Table,—as will be found more particularly adverted to immediately.

2ndly. With regard to the *annual* variations in the level of the Lakes, and their general extent; and how far these also occur simultaneously, and are likewise owing to the amount of rain and snow compared with that of the evaporation; or what other cause:—I am free to confess that, *cæteris paribus*, and in accordance with the various authorities adduced, as well as all other information which I have been enabled to obtain, the same observation must apply to these variations as to the septennial fluctuations just noticed: but that while the extremes between the maximum and minimum range of the great floods may be rated at about six feet, the average difference of level during a single year may be between two and three feet; and that as already stated, though the rise and fall in all the Lakes may usually be simultaneous, one may sometimes be low while the others are high. As, for instance, it will be seen by a reference to the "Miscellaneous Remarks," that in 1795–96, Lake Ontario was so high as to drown trees of many years' growth while Lake Erie was described as so low that the gravelly beach near Cleveland was used as a public road; and that in 1814, "the upper Lakes were full," whereas "the centre and lower Lakes" were not so till the following year; and that in 1827 Lakes Erie and Ontario were between two and three feet above their usual level, while Lake Superior was lower than ever known before;—all which circumstances combined, with others yet to be noticed, have produced a conviction that each Lake is independently liable to irregularities of level peculiar to itself. I allude to the well known, but little thought of, *fact*, that during the winter months large boulders as well as smaller masses of stone and gravel, lying along shore, become firmly imbedded in the bordage ice, and on any rise of the waters, towards the close of the season, remain firmly attached to the moving floating masses, liable to be either dropt again in deep water, on the ice becoming what is called rotten, or to be removed to some distant part of the shore, if not to be carried along by the united wind and current towards the outlet of the Lake. Admitting such to be the case—for there is every year abundant evidence of the fact—it only remains to suppose that towards the end of winter, as frequently occurs, an accumu-

lation of loaded drift ice takes place near the head of the Rapids in the neighborhood of Buffalo, and becomes temporarily united by a fresh frost, and that a *jam*, as it is termed, then takes place, so as to leave a more contracted space than usual for the passage of the rushing volume of water below the broad roofing of ice, and that a further rise afterwards happens, coupled with a thaw, during which a deposition of the hitherto suspended rocky materials takes place at the bottom of the channel; it will then be evident that a still more contracted space will be left for the discharge of the increasing flood; and the natural consequence will be, that after the breaking up of the ice, the general surface of the Lake will have assumed a higher level than would have otherwise been the case, proportioned to the thickness of the stratum of boulders and other rocky materials deposited at the bottom of the channel, —liable to remain for a shorter or longer time, until gradually removed by the action of the sweeping current: a process altogether dependent on the strength of the latter, compared with the degree of compactness and solidification acquired by the rocky barrier opposed to it; and which may therefore require a whole season, or even more, to be accomplished. Of the motive power of ice, I myself have had ample proof, in the frequent dislodgment of boulders of large size from one part of the Lake shore to another, near my own farm; but more particularly of a vast rugged mass of limestone rock moved from comparatively deep water, some distance out in the Lake, to a more shallow part, so near the shore, that a large tree, dislodged from the high bank above by the undermining fury of the waves, happened to fall over in such a manner that its stem formed a very convenient though giddy bridge, from the beach to the stranger rock, and thereby allowed the latter to be afterwards used as a pleasant fishing station by my children. There are also, to my own knowledge, many instances of the removal of boulders in the different parts of the Rapids near Montreal. And among many examples of the almost entire temporary obstruction of the outlets of Lakes Huron and Erie by the jamming of the ice, I shall append to this paper an account of one which took place in the Niagara River, between Buffalo and Fort Erie, in March, 1848, with which I was at the time so much struck that I was induced to write to a friend on the spot for further particulars, in hopes of elucidating my long-cherishsd hypothesis; and such I have no doubt would have been the case had I been able to be present myself to compare facts. Independent of that, however, the particulars connected with the obstruction in the Niagara* alluded to, were of so extraordinary a character as to deserve being placed on permanent record.

* The account of this singular phenomenon is unavoidably postponed till some future time.

With respect to the 3d debatable question—the daily oscillations or other irregular transient tides observable in the different Lakes; I may observe that, making allowance for a greater or less degree of barometrical pressure, I might perhaps be disposed to assent in few words to the now generally received opinion, that in other respects they may be ascribed to the influence of the prevailing winds upon their broad expanse, more or less modified by their peculiar form and direction, and the relative bearing and nature of their extremities, as well as by the often very jagged and irregular outline of particular inlets or bays, and other inter-peninsular localities, such as Kewenaw Bay on Lake Superior, Green Bay on Lake Michigan, Presque Isle Peninsula and Long Point on Lake Erie, and the Bay of Quinté on Lake Ontario. But it seems to me that in so doing I would be conceding too much, as, in my humble unscientific apprehension, I am disposed to think that though such may be the case to a general extent, it is not the less necessary to prove, by a long and regular course of minute observations, whether such be the fact or not, as well as how far the surface of such vast bodies of water may not at times be considerably influenced by barometrical pressure on the one hand, or by lunar attraction on the other, particularly at the times of the vernal and autumnal equinoxes; and the more so, considering that late observations of philosophers in Europe have not only decided that there is a perceptible tide in the Mediterranean, Euxine, and Baltic, as well as in other altogether close saline seas, but also that something like barometric and lunar influence, or both, is observable on the inland fresh-water lakes of Switzerland and elsewhere. In confirmation of this I would, as regards the latter, beg to refer to the writings of Dr. Young, alluded to in an early part of these remarks, in conjunction with a valuable paper on the Lakes of Switzerland, by Colonel Jackson of the Royal Geographical Society, which lately appeared in the *Canadian Journal*, incorporated in a series of interesting articles on the variations in the level of the Canadian Lakes, from the pen of its learned editor, in which those oscillations (there termed *seiches*) are said to amount to no less than five feet. Nay, so interestingly appropriate to the present question do I regard a portion of the article alluded to, that I am tempted, in spite of the already great length of this paper, to transcribe the following, as the conclusion at which a learned German Professor has arrived on the subject:—"1st. That the *seiches* of the Lake of Geneva are much more frequent than is generally imagined. 2d. That they happen at all seasons of the year and at all hours of the day; but that they are generally most severe in the spring and in the autumn. 3d. That the state of the atmosphere seems to have a decided influence, it being remarked, that in proportion as that state is less changeable, so are the Seiches less frequent, and *vice*

versa. The Seiches have always been considerable when the atmosphere has been loaded with heavy clouds, or when the weather, in other respects severe, has threatened to be stormy, and when the barometer has sunk. 4th. That though Seiches are more frequent in spring and autumn, they are more considerable in the summer, and, in particular, towards the close of the season. The highest that have been observed happened in the month of September. 5th. That the minimum of the Seiches has no precise term: their maximum seems to be five feet. 6th. That although the duration of the Seiches is very variable, the greatest extent seems not to exceed 20 or 25 minutes, but usually lasts a much shorter time. And 7th, That they are not peculiar to the Lake of Geneva alone; M. Vaucher having observed them on the Lakes of Zurich, of Annecy, and of Constance."

I cannot refrain from also quoting the following paragraph from the same paper, as much to the point:—

"It appears unquestionable that the phenomenon of the Seiches is due to an unequal pressure of the atmosphere in different parts of the Lake at the same time, *i. e.*, to the simultaneous effects of columns of air of different weight, or different elasticity, arising from temporary variations of temperature, or from mechanical causes; and if such be the case, all Lakes of a certain extent, and even inland seas, must be subject to the same influence, and therefore present the same phenomenon; and I have little doubt that correct observations will verify the presumption."*

With respect to the irregular tides observable in the Baltic and Black Seas, and other great bodies of saline water of a similar character, it will be sufficient to give the following, regarding the first-named sea, from a standard geographical work, as bearing intimately on the subject under discussion:—"The Baltic being a close sea, is of course not subject to the phenomena of regular tides. But though such be wanting, a variation in the height of its waters, equal, frequently, to 3½ feet Swedish, is observed at irregular intervals. This occurs at all seasons, but chiefly in the autumn or winter, at the time of heavy rains, or when the atmosphere is charged with clouds, though unattended with falling weather. The water maintains its height frequently for several days, sometimes even for weeks. Prevalent winds, flooding rains, melting snows, and many other causes were assigned for this very remarkable phenomenon; but it continued to occur, independent of all these, till 1804, when Schulten, a Swedish physician, after having collected all the observations that had been made, found 'that the greatest height of the water corresponds with the greatest depression of the barometrical column; and conversely.' The almost total absence of oceanic action in this

* See Canadian Journal, vol. ii, pp. 27, 28, &c.

sea leaves the cause thus assigned to operate with full power; and if Schulten's hypothesis be confirmed, of which there is now but little doubt, it will, in all probability, serve to explain similar phenomena observed in other close waters, as the Caspian, Lake Balkai, and the Lake of Geneva, to which Saussure has assigned similar causes."*

[An important paper on the fluctuations in the surfaces of the Lakes not referred to by Major Lachlan is published by Charles Whittlesey in Foster and Whitney's Report on the Lake Superior Land District, Part 2, 1851, p. 319. It reviews the facts, adds a large number of observations and sustains the conclusion that the rise is not periodical.—Eds.]

ART. V.—*Remarks on the changes which take place in the Structure and Composition of Mineral Veins near the surface, with particular reference to the East Tennessee Copper Mines;* by J. D. WHITNEY.

IN the number of the American Journal of Science for March last, ([2] xix, 181,) M. Tuomey has given "a brief notice of some facts connected with the Ducktown, Tennessee, Copper Mines." As this notice seems to me to convey a wrong impression with regard to the mode of occurrence of the ore in the veins of the region alluded to, I take the liberty of stating what I conceive to be the correct interpretation of the phenomena displayed on so large a scale in the Polk County, or Ducktown Mines.

Mr. Tuomey remarks as follows: "in every published account of the mines that I have seen, the impression is left, *that the ore* (*b*) *is derived from the underlying portion of the bed* (*c*) *by decomposition.*" No such impression could, by any possibility, be derived from the descriptions of the East Tennessee Mining region which I have published. To show this, I quote from the report† to which Mr. Tuomey refers, at the commencement of his notice, as follows: "On penetrating beneath the surface, the section represented in the annexed figure is obtained. (See section accompanying Mr. Tuomey's article.) Beneath the gossan, is found a bed or mass of black cupriferous ore, of variable thickness and width. This, as well as the gossan, is the result of the decomposition of an ore consisting originally of a mixture of the sulphurets of iron and copper, which was associated with a quartzose gangue or vein-stone. The place of the bed of copper ore marks the limit of the decomposition of the vein; *beneath it the*

* See McCulloch's Geographical Dictionary, Article *Baltic.*

† Report on the Mining tract of the East Tennessee and Cherokee Copper Mining Co.; by J. D. Whitney. New York, 1853.

ore exists in its original condition." It seems hardly possible that any one should ever have supposed the black ore to have resulted from the decomposition of any other part of the vein than that which lies above it, as below it everything remains in its original, unaltered condition.

The decomposition of metalliferous lodes in their superficial portions is a matter often noticed and generally expected by the miner, and there is nothing anomalous in this respect in the East Tennessee Copper region. The commonly observed facts are these: the predominating metalliferous ores which are wrought in mines, especially of silver, copper and lead, are sulphurets, sulphur being the usual mineraliser, although arsenic and antimony are not unfrequently found in connection with sulphur in combination with these metals. These ores are sometimes scattered irregularly through the gangue in fine particles and sometimes arranged in nearly parallel bands or plates, which are separated from each other by belts of barren veinstone. This is the normal condition of the veins at a considerable depth and some of them retain their original appearance and remain chemically and mechanically unchanged up to the very surface. In most metalliferous lodes, however, it is found that the ores have undergone decomposition down to a certain depth, which rarely exceeds 300 feet and generally falls between 50 and 100 feet. This decomposition is perhaps more common and more strongly marked in cupriferous lodes than in those of the other metals, although some argentiferous veins in South America exhibit it on a grand scale. The predominating ores of copper are the variegated ore and copper pyrites, both of which are combinations of sulphur with copper and iron and their presence in the veins beneath is indicated on the surface by an outcrop of what the Cornish miners call *gossan*, a term which has been generally adopted wherever the English language is spoken. This is a hydrated peroxyd of iron, usually much mixed with silicious and earthy matter and having a somewhat open and porous structure. Associated with this ferruginous mass the oxydised combinations of copper are often found occurring, at no great distance from the surface; among these, the carbonate and silicates are the most common, the phosphate and arseniate less so. The oxyds themselves and the native metal are among the products of decomposition. Sometimes these oxydised ores are very abundant in the upper part of a cupriferous lode, and are wrought with large profits, owing to their richness and the softness of the ground and the consequent facility in mining. In other localities, nearly all the copper has disappeared from the upper portion of the vein and only traces of these ores are found with the gossan. On sinking down into such decomposed veins a gradual change is found to take place in their character: the oxydised ores are replaced by the sulphurets; the

ferruginous aspect of the lode disappears; the gangue becomes more solid, and the walls are better defined.

These changes in the upper portion of the sulphuret-bearing lodes are usually conceived to be the result of the action of air and water introduced from the surface and penetrating gradually downwards. Through their joint influence the sulphuret of copper and iron is gradually decomposed and while the latter metal remains behind in the form of an impure hydrous oxyd, or gossan, the copper is also converted into an oxyd and may remain in that state, or combine with the sulphuric acid furnished by the oxydation of the sulphur of the original ore, or with any other acid which may chance to be present, thus giving rise to the numerous beautiful ores, most of which contain water, which are so common in the higher portion of cupriferous veins. The nature of the combinations resulting from any such decomposition and their relative quantity must, of course, depend on the quantity and quality of the ore originally in the lode, the proportion and kind of vein-stone and probably still more on chemical and perhaps electric agencies, the precise mode of action of which is as yet but imperfectly understood.

In the Polk County mines, these changes are displayed on a grand scale. The metalliferous veins which belong to the segregated class, are very wide and the decomposition has been very complete, so that the outcrop of gossan is very marked and in some places occupies a width of 100 feet on the surface, consisting of large angular blocks of ferruginous rock piled up along the line of the vein. On sinking into this mass of ferruginous matter it is found to be tolerably soft, but at the same time so compact that excavations in it need but little timbering. If the shaft is commenced on the summit of a hill, it will be necessary to penetrate a hundred feet, perhaps, before any change in the nature of the vein is perceived. In the valleys the distance required for this purpose is much less. The depth at which the gossan terminates is nearly coincident with the water-level, or the point where, in sinking, water is found in considerable quantity. Here a layer or bed of copper ore is met with of very irregular dimensions, in some places occupying large bunches or pockets of many cubic yards in content, and in others forming only a thin stratum. This deposit of ore is quite as variable in composition as it is in dimensions. Its color is usually quite dark, and when rich in copper, almost black. It is evidently a mechanical mixture of black oxyd of copper with sulphurets of iron and copper, sulphate of copper, oxyd of iron, silicious matter, and some manganese. The per-centage yield of copper is usually low; but the purest portions contain from twenty to thirty per cent. of metal. This deposit of black ore is the object of exploration in the mines, and the only source, thus far, from which copper has been obtained in any quantity worthy of notice.

Beneath the black ore is the undecomposed portion of the vein, showing, in two or three points, where I was able to see it at the time of my visit (1853), a hard quartzose gangue with particles of copper pyrites scattered through it, and associated with a considerably larger quantity of iron pyrites. There seems no reason to suppose that the ore which originally existed in the upper part of the vein, from whose decomposition the black ore was derived, was any different in nature from that found below, although there may have been bunches of it considerably richer in copper. The deposit of black ore is insignificant in dimensions, compared with the mass of gossan which overlies it, and when we consider that a large portion of the copper which was once disseminated through perhaps a hundred feet of overlying vein-stone is now concentrated into the thickness of perhaps two or three feet, on an average, it will be seen that it is not necessary to suppose that the whole of that portion of the vein which is above the bed of black ore, "doubtless once consisted of yellow sulphuret of copper," as Mr. Tuomey supposes to have been the case. Certainly there is no reason to believe that the black ore is a sulphuret of copper altered by heat. Apart from the consideration that it is not such a product, or mixture of products, as would be produced by any igneous action on copper pyrites, we can conceive of no way in which the effect of increased temperature could be limited to the upper portion of the vein, so that that only should undergo decomposition. That the changes in question are exclusively the result of a humid process, can, as it seems to me, be hardly doubted. The concentration of the black ore in one stratum seems to have been due to the percolation of the surface water which was constantly carrying it downwards to the point where it was stopped by the solid portion of the vein.

That the subject of the decomposition of veins is one which is thoroughly understood should by no means be inferred from the preceding remarks: there is, on the contrary, much in these phenomena which has not, as yet, been satisfactorily explained. We know, indeed, that the changes of the sulphurets with oxydised combinations do occur, for we see them taking place under our own eyes, through the joint action of air and water holding carbonic acid in solution; but why in some mining districts the metalliferous veins should be thus effected, while in others no change whatever has occurred, is less easily understood. Burat has called attention to this circumstance, and cited some instances in which the sulphurets remain entirely unoxydised up to the very surface. Thus the cupriferous veins of Mouzaia in Algiers project out from the surface like walls, being more permanent than the adjacent rock and the first blow of the hammer reveals the pyritiferous ore in its natural state. The same thing may be observed in this country. Throughout the Northern states the pyritiferous lodes remain apparently in their unaltered condition;

or, at most, have undergone but little change and exhibit hardly any indications of gossan. The enclosing rocks are not at all softened or stained with ferruginous matter. As examples of this he would instance the great veins of Shelburne, and Eaton, in New Hampshire; those of Hampshire County in Massachusetts; and the St. Lawrence County mines in New York. In none of these has any marked change taken place near the surface. In one part of the Southampton (Mass.) lode a few oxydised ores were found when the mine was first opened, but they were but small in quantity compared with the mass of the unaltered ore. This state of things is a great drawback on the opening of the New England mines since the expense of sinking and driving in the hard granite and quartzose rocks is enormous. In North Carolina, South Carolina and Georgia, on the other hand, the gneiss and slates are often found over a great extent of territory completely decomposed and softened, so that they may be excavated with the pick and shovel, down to a depth of fifty or a hundred feet. I have known a shaft sunk in North Carolina in the rock to the depth of sixty feet in one week.

In the veins of that State, the principal, indeed, almost the only, one near the surface is an auriferous gossan resulting from the decomposition of iron pyrites, with which a little copper pyrites occurs intermixed. Of this latter ore, the quality in several instances seems to increase with the depth of the workings. If the veinstone is wholly quartzose the extent of the decomposition is much less than when it contains feldspathic or slaty portions. Thus in the McCullock mine, in Guildford County, N. C., there is a body of soft ferruginous ores containing a good amount of gold which extends downwards more than 100 feet and parallel and coëxtensive with this auriferous mass, which may be mixed with a shovel, there is a heavy bed of quartz with iron and copper pyrites scattered through it, in which no traces of decomposition can be perceived.

With regard to the East Tennessee veins the practical question of the most importance is: what kind of ore and how much of it is likely to be found in sinking into the undecomposed veins beneath the level of the black ore. This, we believe, can only be determined by actual trial. If in cleaning out the deposit of ore, which lies upon the hard veinstone beneath, there should be bunches of cupriferous ore found, the best of them should be opened by sinking on them, and there can be no satisfactory reason given, based either on analogy or on the appearances of the bodies themselves, why considerable quantities of the yellow ore of copper should not be found within a reasonable depth. Still it is not impossible, that, as these views do not exhibit the characteristics of true fissure veins, they may be found to have been richest near the surface and not to be capable of being worked with profit in the hard rock.

ART. VI.—*On a Universal Indicator for Microscopes;* by Prof. J. W. BAILEY, U. S. Military Academy.—With a Plate.

IN the Quarterly Journal of Microscopical Science, vol. i, p. 34, an ingenious contrivance for registering the position of microscopic objects is described by Mr. Tyrrel; a modification of this, by Mr. Aymot is given in a subsequent number (l. c., vol. i, p. 301); and a still better arrangement for the same purpose, suggested by Mr. Brodie and applied by Mr. Okeden to his microscope, is described at p. 166 of volume iii. of the same work. The last mentioned device can scarcely be improved upon for convenience; but there is one defect which is inherent to all these inventions, viz: that they are essentially selfish contrivances, of no use to any one but the owner of the particular instrument to which they may be attached.

The object of the instrument I propose is more comprehensive than that of the "Finders" above alluded to; being no less than to make a Universal Indicator by means of which an observer can so register the position of any number of objects mounted upon slides, that when these are sent to a distant correspondent the latter may be able by means of the Indicator to find at will any of these objects as easily as if he had the identical microscope and "Finder" by which they were at first recorded. If such a mode of recording the position of objects can be generally adopted that when the register is once made, the record and the objects shall then be entirely independent of the original instrument and observer, and applicable to any microscope, it will tend to promote science not only by facilitating the interchange of specimens among naturalists, but it will give to each observer's collection when properly registered, a permanent scientific value and utility which it could have in no other manner.

The plan I have adopted is to make upon an engraved card what may be considered as a transferable stage having guide lines by means of which the centre of the field of view of the microscope, and the position of a slide when any object upon it occupies this centre, may be given.

Plate I. shows the Indicator complete. The centre of the field of view corresponds to the intersection of the horizontal line C, D, with the vertical line E, F. On the right and left hand of this centre the vertical axes B, and A′, are placed at distances of $\frac{4}{5}$ths of an inch, and the axes A, and B′ are similarly placed at the distances of $\frac{8}{5}$ths of an inch from the centre.

The axes are then graduated as seen in the plate; the small divisions being each $\frac{1}{5}\frac{1}{0}$th of the standard inch.

The dotted lines G, H, I, give the outline of what will be referred to as the *centre piece.*

Should it ever be desired to reproduce the Indicator by engraving or otherwise, the dimensions above given must be most accurately preserved. The dimensions here given were taken from the standard inch of the United States belonging to the State of New York and preserved in the office of the Superintendent of Weights and Measures in Albany. It is the same as the English inch.

The slides on which objects are mounted to be used with the Indicator must have *guide-lines* ruled on their *under side* as shown in fig. 1 and 2. The horizontal line parallel to the lower edge and passing through the middle of the slide is not continued over the portion of the slide which is to be occupied by the objects and their glass cover. The distance of each of the vertical lines from the middle point of the slide is *one inch*. Great accuracy in the distance between these lines of the slide is not essential when they are to be used with the ordinary form of the Indicator as above given, but it is desirable when they are to be employed as hereafter described with a modification of the Indicator applied to a moveable stage.

The slides should all be marked with an arrow placed upon their upper and right hand corner, as shown in fig. 1 and 2, to point out the edge which must always be kept in front in using the slides upon the Indicator.

The Indicator is to be used as follows:—Cut out the centre piece with a thin bladed knife following the outline G, H, I; then replace the piece cut out, and make a hinge for it along the line G, H, by pasting underneath it a piece of thin paper which will bear repeated folding, so as to connect it to the rest of the card.

The Indicator being now ready for use, it must be firmly secured to the stage of the microscope in such a position that its centre as given by the intersection of the lines C, D, and E, F, when viewed as an opaque object, may be exactly in the centre of the field of view. If the stage is a moveable one, it must be kept stationary after the Indicator is properly centred.

The Indicator having been adjusted as above directed, the centre piece is to be turned down and the instrument is then ready for use, either to record new objects, or to find those previously recorded. The slide is to be put upon the Indicator, and guided either by the fingers or a moveable ruler so that when any object which is to be registered occupies the centre of the field of the microscope, the horizontal guide line upon the slides shall pass through the same numbers on two vertical axes of the Indicator as remote from each other as possible. In some positions of the slide the axes A, and B′ can be used for this purpose; in others A, and A′, or B, and B′ must be employed.

The horizontal line of the slide being arranged as just directed it will be found that at least one of the vertical guide lines of the

slide will intersect the horizontal graduation. By observing now the numbers at which the guide lines respectively stand, the record can be made. Suppose for example that the horizontal guide line ruled upon the slide intersects the verticals of the Indicator at 43, while the right hand vertical of the slide cuts the horizontal series of numbers of the Indicator at 75; the entry to be made for this object in the register would be written thus $\frac{4}{7}\frac{3}{5}'$ and whenever that particular object is to be found either by the same Indicator or *any other copy of it*, if the slide is placed at these numbers and the Indicator is properly centred, the object must be in the field of view. In the same manner any number of objects can be registered or found. If the slide happens to be so placed that both of its verticals intersect the graduated portions of the horizontal line C, D, the position of either one of them can be recorded at will.

If a guide line upon the slide falls between two divisions of either scale, the fraction of the division may be estimated with sufficient accuracy by the eye or a hand magnifier and entered in the register. Thus the recorded position $\frac{25\frac{1}{8}'}{34\frac{1}{4}}$, would mean that the vertical lines of the Indicator were intersected at $\frac{1}{8}$th of a division of the scale beyond 25, while the vertical guide line of the slide passed $\frac{1}{4}$th of a division beyond the number 34 of the horizontal scale, as nearly as could be estimated.

It is convenient to let the lower edge of the glass slide rest against a straight edged guide piece which can be moved parallel to the horizontal line of the Indicator. By pushing the slide along this edge, all the objects on the same horizontal can be found without changing the position of the guide piece. By moving the guide piece a little forward or back another sweep across the slide may be made, and so on until every object of interest is found.

By following the directions above given it will be found that the recording or finding of objects by means of the Indicator is very easily performed, and scarcely requires the time which has been employed in describing the method. It is believed that the explanation above given is sufficiently explicit to enable any one to use the Indicator; but some additional remarks will now be made upon the reasons for adopting the particular arrangement I have used, the modes of insuring accuracy in manipulation, and the modifications of which the Indicator is susceptible for individual convenience.

It was desired to make the instrument capable of universal application; so simple that it could be adapted to any stage; so light and yet so strong that it could be sent without injury by mail or otherwise to any distance; and lastly that the different copies should be perfect fac-similes of each other and reproducible

at any time. All this is secured by having the Indicator engraved upon a steel plate and printed upon cards of uniform quality, and by taking the dimensions from the standard United States inch, preserved in the office of the Superintendent of Weights and Measures in Albany. In order to extend the use of the Indicator to all cases which are likely to occur, the graduation was arranged with reference to slides three inches long and one inch wide, while it will answer equally well for smaller ones. When these slides are not covered with paper, and guide lines can be ruled as above directed upon the glass itself, the graduations necessary for their use would only extend upon the verticals $\frac{1}{2}$ an inch above and below the horizontal line, and upon the horizontal line only $\frac{1}{2}$ an inch outwards from the points 40 and 70; but in order to provide for paper covered, or opaque slides whose uppermost and lateral edges may be used as guide lines, the graduation has been extended considerably. It will be found on trial that slides of the standard size, whether paper covered or not, may be employed with the Indicator for the registration of all objects under a glass cover of a square inch in size, which is quite as large as is likely to be used. In using covering glasses of an inch square it will be found necessary to employ the horizontal numbers 0 to 50, and the verticals A, A′, for objects towards the right of the cover, and the other axes and numbers for objects towards the left. For a portion of the objects under the cover either set of axes and numbers may be used at pleasure provided that the verticals are chosen as far apart as possible.

Two verticals on the same side of the centre should never be used together, as a small error in observing the numbers would have more effect in displacing the object from the centre than if two axes at a greater distance had been employed. The reason for leaving a blank ungraduated space between 50 and 60 on the horizontal line was to allow a fac-simile of the Indicator to be engraved upon the stage of any microscope, the blank space being left for the portion of the stage occupied by the aperture.

The guide lines upon the glass may be ruled with a fine pointed scratching diamond, and be rendered more visible by having graphite or black lead rubbed into them. Lines ruled in this manner will answer for all except very minute objects, but in consequence of the widening of the lines by the chipping up of the glass due to unequal expansions and contractions, the lines often become too wide and irregular for very accurate adjustments. In such cases admirable guide lines may be etched upon the glass with the *vapor* of fluohydric acid and can be made of any required degree of fineness. The *solution* of the acid should not be employed for the etching as it gives lines which are too smooth and difficult to see, and which will not retain the black lead if rubbed into them.

The power of the objective employed in determining the position of an object for registration, should always be the highest which can be conveniently employed; while in searching for an object already recorded, a power lower than that employed in the registration may be used. The object then must be in the field of view, and would be at the centre but for slight errors in manipulation, or the want of perfect adjustment in the mountings of the object-glass. Care should be taken to bring each object accurately to the centre of the field of view, before recording it. It will then require an error equal to half the diameter of the field of view to throw it out of that field. For example the field of view of my $\frac{1}{2}$ inch objective made by Spencer, includes two divisions of the Indicator, and hence an error of nearly one division might be made in placing a slide upon the Indicator by means of its recorded numbers and yet the object would be found in the field.

It may happen that in transferring a slide from one Indicator to another that the object when brought into sight by means of its recorded co-ordinates will not appear well centred. If this be due to slight differences in determining the centres of the Indicator, and yet the record has been carefully made, it is easy to correct for the difference in the following manner. Move the Indicator *with* the slide placed at any recorded position until the object comes into the centre of the field of view, then secure the Indicator to the stage in this new position, and all other objects recorded by the same Indicator ought to be brought to the centre of the field of view by means of the numbers as registered.

The convenience of the Indicator for individual use may be increased by several slight changes. One of these consists in removing the paper centre piece, and replacing it either temporarily or permanently by a glass plate bearing lines at right angles to each other ruled very lightly with a diamond point, and so adjusted as to coincide with the prolongation of C D and E F through the centre. For all but the highest powers there is no objection to having these excessively minute lines permanently beneath the centre of the Indicator as they do not perceptibly interfere with the light, and it is convenient to have them always in place. They can be ruled upon a piece of mica or thin glass cemented to the back of the Indicator, or the latter may itself be cemented to a piece of plate glass and the central guide lines then carefully ruled. Even for the highest powers these lines can be used in recording the position of objects, which can then be found for study by using an Indicator of the ordinary form. By a proper arrangement, a moveable stage, with screws for vertical and horizontal motions, may be graduated so as to correspond to the Indicator, and yet preserve all the advantages of accurate adjustment which the screws afford. For this purpose it is necessary to ob-

serve that if the Indicator be placed upon the stage and accurately centred, with its guide line C, D, parallel to the front edge of the stage, and a slide be then placed upon the Indicator so that its horizontal guide line shall coincide with C, D, and the right hand vertical guide line stand at 70, (*i. e.* in the position which would be recorded as $\frac{5}{7}\frac{0}{0}'$,) or its left hand guide line at $\frac{5}{4}\frac{0}{0}'$; then a motion of the stage itself bearing with it the Indicator and slide, or an equal motion of the slide upon the Indicator and fixed stage will bring the same point of the slide to the centre of the field of view.

Therefore by attaching to the stage in any convenient manner graduations corresponding to those of the Indicator, and by having lines corresponding to $\frac{5}{7}\frac{0}{0}'$ and $\frac{5}{4}\frac{0}{0}'$ ruled upon the stage, it will only be necessary to place the slide directly on the stage at these numbers, the stage itself being set either at $\frac{5}{7}\frac{0}{0}'$ or $\frac{5}{4}\frac{0}{0}'$ of its graduations. By turning the milled heads of the screws which give the vertical and horizontal motions of the stage, the object can be brought into the field of view and recorded or found again by means of the numbers attached to the stage; while the record may be used for any other Indicator as if made in the usual manner. If the distance between the guide lines upon the slide agrees accurately with that between 40 and 70 of the Indicator, the slide when placed upon the moveable stage at either $\frac{5}{4}\frac{0}{0}'$ or $\frac{5}{7}\frac{0}{0}'$ will need no displacement for the whole series of numbers; but if this distance does not agree, the slide must be put with its left hand vertical coinciding with the left hand vertical of the stage for all numbers from 0 to 50 of the horizontal series; while from 60 to 110 of the same series the slide must be set so that its right hand vertical coincides with the right hand vertical of the stage; in each case the horizontal lines of the stage and slide being adjusted to coincide. By observing this rule the necessity of perfect accuracy in the position of the guide lines upon the slides is done away with.

There are some objections, but not insuperable ones, to the moveable stage Indicator as above described. In the first place the stage as usually made has its motion too limited to correspond to the whole range of the Indicator, and secondly the guide lines ruled upon the stage for one object-glass may not answer for other powers on account of slight inaccuracies of mounting.

The stages can doubtless be constructed to give as wide a range for motion as required, which will do away with the first mentioned objection. The second may be removed by placing an Indicator upon the upper plate of the stage when the latter stands at $\frac{5}{7}\frac{0}{0}'$ and adjusting it so that when well centred *for the power employed* the line C F shall be parallel to the front edge of the stage. The slide being then placed upon the Indicator with its guide lines at $\frac{5}{4}\frac{0}{0}'$ or $\frac{5}{7}\frac{0}{0}'$ the remaining motions may be made with

the screws in the usual manner and the numbers may be read off from the stage scales instead of the Indicator.

The above mentioned modifications are excellent for individual convenience; but for the general purposes of science, the comparable, transferable, reproducible Indicator, in its simplest form, must be preserved; and it is only in that form that it deserves the name, suggested by a friend, of the Universal Indicator.

As a proof of the utility and accuracy of the Indicator, and of its convenience as a means of scientific exchange, I may state that numerous mounted slides of minute recent and fossil Diatoms have been exchanged through the Post Office by Judge A. S. Johnson of Albany, and myself, and that each has found by the ordinary as well as modified forms of the Indicator all the shells however minute, fragmentary or previously unknown, which the other had recorded. Some of these objects were less than a $\frac{1}{1000}$th of an inch in diameter, and yet they were found without difficulty by means of the Indicator.

To determine whether different impressions of the Indicator when made on the same kind of paper were comparable, a set of objects was registered successively by seven different impressions made on enamelled cards some of which were arranged with the ordinary paper centre piece, and others with the central guide lines ruled upon glass. The numbers being recorded for the objects when well centred upon one of these Indicators, the slide was then transferred to each of the other Indicators and each object being brought into the field by its recorded numbers, the position was carefully adjusted so that the object should be well centred, and a record for each copy of the Indicator was thus made. On comparing the different numbers it was found that the coincidence was almost perfect, the difference never exceeding $\frac{1}{4}$ of one of the divisions of the Indicator, an amount which might be quadrupled before an object would be thrown out of the field of view of my $\frac{1}{2}$ inch objective.

The Indicator having been put to so many and such severe tests I feel no hesitation in recommending it as a means of scientific intercourse among observers, and as a means by which collections of microscopic objects may be registered, arranged, and catalogued; and an index to the whole so made that any particular specimen may be found at will either by the original observer or any one into whose hands the slides and accompanying register may at any time come.

The copy of the Indicator which accompanies this paper is not given for use with the microscope, as the kind of paper upon which it is printed is different from that used for the standard Indicator, and therefore in consequence of unequal shrinkage a slight deviation is produced. The Indicator for use with the microscope

is printed upon enamelled cards, and the different impressions have been found to agree so closely with each other as well as with the original plate that no appreciable error is perceived.

I can not close this paper without expressing my warm thanks to Judge A. S. Johnson of the New York Court of Appeals, for his cordial sympathy and aid in testing the merits of the Indicator and for some excellent suggestions as to its best form for general use. I should also express my obligations to the engraver, J. E. Gavit, Esq., of Albany, who has spared no pains in making the steel plate from which the Indicator is printed as accurate as possible.

ART. VII.—*On the Composition of Eggs in the animal series;* by VALENCIENNES and FRÉMY.—Part III.*

Eggs of Reptiles.

IN the two preceding memoirs, we have given the results of our observations on the eggs of Fishes. We have pointed out in these eggs the presence of the proximate principles, ichthin in Rays and Sharks, ichthulin in the species of the numerous families of osseous fishes, and have directed attention to the important fact, that this principle is gradually modified in proportion as the egg approaches its maturity, and that it ends by disappearing in the egg when it is ready for fecundation, the ichthulin being then replaced by albumen.

We now continue the account of our researches on the eggs of other oviparous animals.

Eggs of Tortoises.—We have examined the eggs of two species of Chelonian reptiles belonging to two distinct genera. Some were hatched by a land Tortoise from Algiers, which M. Dumèril has called *Testudo Mauritanica.* This female has lived for many years in France, at the house of a resident of Grandville. The second is the fresh-water Tortoise of Europe, the mud Tortoise (la bourbeuse) of Daubenton and Lacépède, which M. Dumèril calls *Cistudo Europæa.* Although these two species of Chelonians inhabit different countries and climates, the resemblance in the constitution and composition of the liquids of their eggs is well worth remarking; but there is a further similarity which we hardly expected; it is that they have a great analogy to those of cartilaginous fishes. They are formed of a whitish gelatine, not very abundant, hardly albuminous, shut up in the cells of large transparent membranes. The yellow very rich in albumen, contains besides a considerable quantity of phosphuretted oil, and also peculiar grains constituting an entirely new principle. These eggs do not contain a material precipitated by water analogous

* From the Journal de Pharmacie for August, 1854: translated by Dr. Rosengarten. For Parts I and II, see last volume, p. 38, 238.

to ichthulin. On treating their yolk with water, the yellowish oil of the egg is seen swimming on the surface of the liquid, and is precipitated in little white grains. The water retains in solution albumen and the salts. The little grains can be easily purified by washing in water, alcohol and ether. By these processes a material is obtained which presents, when in great purity, a certain analogy to ichthin, but which differs by characteristics that prevent its being confounded with the substances from the eggs of fishes. We have given to this new substance the name of Emydin.

Emydin.—The grains of this substance are rounded or a little ovoid, and covered with little roughnesses. They are white, transparent, harder and denser than the grains of ichthin. The largest grains of emydin taken from the eggs of the Mauritanian Tortoise (Testudo Mauritanica) are 10 hundredths of a millimeter. They appear enveloped in a very thin membrane, which has the appearance of tearing when compressed under the microscope, or which shows, by its transparency, a sort of nucleus frequently of a hexagonal form. This nucleus increases gradually, its angles disappear, and it ends by becoming spherical. Other granules break as they expand or swell out. They are of all sizes, from one hundredth of a millimeter to very large dimensions. The grains of the *Cistudo Europea* Dum., are smaller than those of the land Tortoise, for the largest did not exceed 6 hundredths of a millimeter, and they appear in general, to be more spherical than those of the other species. Although resisting under the pestle, we have broken them, and found that they break into spherical fragments, from the circumference to the centre. Potash, well weakened, immediately dissolves emydin, while it acts quite feebly on ichthin. Acetic acid, which, as we know, dissolves ichthin with the greatest ease, simply swells the grains of emydin, but does not dissolve them. These characteristics seem to establish very clear differences between ichthin and emydin. Its grains are dissolved in boiling hydrochloric acid, without giving to the liquid any violet color; this action proves that emydin is not the vitellin of bird's eggs.

Emydin submitted to analysis, shows the following composition:—

	Proportion of azote.	Per-centages.
Solid matter, 0·313	Solid matter, 0·370	Carbon, - - 49·4
Water, 0·210	Azote, 0·0579	Hydrogen, - - 7·4
Carbonic acid, 0·568		Azote, - - 15·6
		Oxygen and phosphorus, 2·67

This substance appears to us to be isomeric with ichthin. The grains of emydin leave, on burning, a residue of calcareous salts which never exceed 1 per cent. We have also established that the eggs of our Testudo Mauritanica from the same laying present remarkable differences. The yellow of certain eggs did

not contain grains of emydin; but in that case, the white, in place of being transparent and colorless, had a slightly yellowish tinge. There were visible in it, under the microscope, little grains of irregular form which seemed to have some analogy to the grains of emydin of the vitellus.

Lizard's Eggs.—According to our observations on the eggs of the Lacerta "vert piquetè," and on those of the Lacerta stirpium (Lezard des souches), their vitellus presents a certain resemblance in composition to that of the yolk of birds. We found neither grains of ichthin nor emydin.

Adder's Eggs.—We have examined eggs of the Ringed adder (*Coleuvre à collier*), and those of the Esculapian adder. Our analyses confirm those of MM. Martin-Saint-Ange and Baudrimont. We found the vitellus of these eggs scarcely surrounded by a very thin layer of albumen. The yellow is formed of albumen and of phosphuretted fat, and it appears to throw down vitellin when washed with water.

Viper's Eggs.—The eggs of the common viper differ in several ways from those of the Adder. They contain a white, like that of the Ray, of a gelatinous consistency; it is formed by elastic hyaloid membranes, with a slightly albuminous fluid. The yellow of the Viper's egg, presents itself, like that of the adder, in the shape of a very albuminous liquid, holding suspended in it a considerable quantity of fat; when this yellow is put into contact with water, it produces a phenomenon that we have remarked in no other species of eggs. The liquid, at first quite fluid, thickens gradually, and soon becomes completely gelatinous. This change of state in the liquor is due to a body like vitellin, found at first in solution in the albumen, which becomes insoluble under the slow action of the water, and finishes by making it harden. Is this vitellin, or something different? We were not able last year to resolve this interesting question; we hope to do it, if we are fortunate enough to get a sufficient quantity of Viper's eggs. We shall also during the present season continue to study the eggs of the Sauria and the Ophidia.

The Eggs of Batrachia.—The numerous observations made upon the eggs of Batrachia have not yet established the nature of the substances which constitute them. The gelatinous matter of the eggs of the Frog is formed of a tissue of hyaloid membranes, containing water absorbed after hatching. It is modified by cooking, becoming slightly opaque, and the vitellus hardens, which proves the existence of albumen. The vitellus blackened by a black pigment peculiar to these eggs and coloring the membrane, which is excessively thin but may be recognised by its dryness, contains in its liquid, a very small quantity of fatty matter united in transparent drops, a matter which looks under the microscope like a black punctation of extreme fineness, and then an

abundance of vitellin granules, very small, transparent, varying in form, more commonly rounded, insoluble in water and soluble in acetic acid. We repeated this experiment several times before being certain of its character. The eggs of the *Tritons* show a like composition. The exterior white is like that of the Frog's egg. The citron or green vitellus, according to the species, contains fat and a considerable number of vitellin granules quite rounded, which we have studied in the egg taken in the oviduct near being hatched, and in the ovula still enclosed in the ovary. The examination of these last has shown us this remarkable fact, that the vitellin granules increase with the age of their formation. They are very much smaller in the ovula than in the vitellus of the egg. These granules are also insoluble in water and soluble in acetic acid. The egg becomes hard when cooked in boiling water; it then contains albumen too, as well as that of the frog. We have already mentioned that MM. Martin-Saint-Ange and Baudrimont saw the granules of the Frog; they have even given their figure. M. Rud. Virchow has also given some accurate observations on the granules of Frog's eggs, Toads and Tritons. (Zeitschrift für Wissenschaftliche Zoologie, par MM. Siebold and Kölliker, vol. iv, part 2, p. 236; 1852.)

The characteristics which we describe, lead us to the conclusion, that the granules contained in these eggs are of the nature of ichthin, that is, that they are of a like nature with those of the Ray's and the Shark's eggs. The simple view of the grains under the lens of a microscope suggested this idea; their characteristics confirm the identity, which has brought us to the establishment of this curious and important physiological fact, that the Batrachians besides undergoing, in consequence of their metamorphosis, a primary condition of existence like that of fish, lay eggs whose composition has the greatest affinity to those of fish.

This similarity holds even in the ovules, for we have already remarked that the granules of the ichthin from a Ray are smaller in the ovules than in the yolk of the egg of these fish. We there show, in fact, a like composition for the white surrounding the vitellus, and the presence of ichthin, that immediate new principle abundant in the egg of cartilaginous fishes.

M. Virchow whom we quote, has likewise observed the granules (Dotterplättchen) of the eggs of Ray and of fish (loc. cit.). We should mention too, that J. Müller has seen and drawn the granules of the smooth Ray and of the smooth Hound-fish (*Galeus lævis*) (Dotterkörner), Mem. Acad. de Berlin, tome xxvii, page 221, pl. 5, 1842.)

We shall give, in the complete work which will be published with plates in the Archives du Museum d'Histoire Naturelle, a detailed account of the researches of our predecessors.

Of the eggs of Crustacea.—Coloring matter of the Crustacea. The Crabs of our soft waters and Lobsters have supplied the eggs necessary for our researches. Lobsters, carrying from fifteen to twenty thousand eggs under their abdominal appendages, are the most convenient for the kind of researches which we have undertaken.

Their eggs do not contain ichthulin; no sort of granules are found in them. They are essentially formed of an albuminous and saline liquor, holding some fatty bodies suspended in it. The albumen of the eggs of Crustacea seems to us different, in some respects, from the albumen of other eggs. Its coagulation begins about 74° C.; the study of this substance will necessarily find place in the work which we are now preparing on albuminous substances.

We have also studied the Sea Lobster (Palinurus). This Crustacea, as plentiful as the ordinary Lobster (Homardus) on granite shores, and not touching the chalky cliffs, is very uncommon in the North. The Sea Lobster does not seem to pass the Islands of Ushant, and is not found in the British Channel. Further, it is very common on the rocks of Bretagne. It lives at a much greater depth than the common Lobster, for it is necessary, in order to take them, to let down the baited hooks to a depth of seventy fathoms. The eggs of these Crustacea are very small, hardly as large as a seed of the poppy. We have counted about 130,000 of them under their abdominal appendages. We had, in the early part of March, a living Sea Lobster, whose eggs were so developed that the two black eyes of the little fœtus were distinguishable through the shell. We have to regret that we could not save it alive, so as to see the young hatch, to follow the phases of their metamorphosis. Science already possesses some observations made on the embryo of the Lobster, but it has none as yet registered on the development of the eggs of the Sea Lobster, and many other Crustacea.

The study of the eggs of Lobsters enabled us to obtain in a state of purity, that curious matter which takes a red color when the shell of Crustacea is submitted to the action of heat. Till now it has not been conveniently studied, because the solvents always presented it in a state mixed with fatty bodies, and besides, the solvents such as ether or alcohol, give it in the reddish condition, that is already modified; it exists dissolved in the albumen of the eggs of Crustacea; by heating the liquid, the albuminous matter coagulates, carrying with it, in the shape of a lake, the coloring matter, which is then of a very beautiful red. The precipitate is retaken by the alcohol, which dissolves the coloring substance, and leaves the albumen in an insoluble state. The detection of this coloring matter, in the egg of the Crustacea, is undoubtedly an interesting fact, if it be remembered that

art has already taken advantage of this substance. The following method enabled us to obtain the coloring matter as it exists in Crustacea, still presenting its green color.

The green coloring matter of Crustacea is soluble in albumen; besides, when Lobster's eggs are crushed, the albuminous liquid which passes by filtering, is strongly colored green, and holds in solution the coloring matter. The ordinary methods, such as the action of heat, that of neutral solvents, desiccation, &c., which are applied to extract this singular substance, present it already modified. In fact, when alcohol or any other agent is used, the substance which till then, was green, instantly turns red; but by an unexpected and for our work, very fortunate circumstance, the only liquid which does not alter this coloring matter, that is water, enabled us to obtain it in a pure state; in fact, when the green albuminous liquor mentioned above, was weakened with a large quantity of water, the coloring matter is precipitated, and can easily be gathered on a filter. We have thus been enabled to establish the following properties, which we think, are of a nature to interest men of science. This substance is green, resinous and uncrystallizable; it changes and becomes red under very curious circumstances. To effect this transformation, it suffices to submit it to desiccation, even at the ordinary temperature. The salts which have affinity for water can transform the green coloring matter of Crustacea into a red substance: the salts, on the other hand, which do not combine with water, exercise no action on this singular coloring matter. The action of a vacuum rapidly produces the red color. Simple rubbing immediately reddens the green substance. Alcohol, ether, the acids, effect the same change. There are not, to all appearances, in the vegetable or animal organization, coloring substances comparable with this of the Crustacea; modifying with such facility under the action of the simplest agents. We tried to determine whether this substance presented the same characteristics when still fixed in the shell of Crustacea; the result of our experience is, that in this case, the green substance bears itself just as it does when isolated. Thus a Crab's shell, which shows the green discoloration, becomes red the moment it is rubbed with a hard body. It is not the heat developed by the rubbing which effects this singular modification, for it has been effected while the shell was very damp, and besides, it is manifested rapidly on a Crab's shell placed under the receiver of an air-pump and submitted to the action of the vacuum. The change of color by rubbing explains how anatomists who have sought for the cause of the discoloration of Crabs when boiled, have always seen the red matter under the coat, as thin as the epidermis which they removed to observe the shell under the microscope. The simple rubbing of the utricles, touched by the scalpel, sufficed to make the green color red. We are fortunate

thus to have been able to complete the history of one of the most curious coloring matters produced by animal organization.

Of the eggs of Arachnids and Insects.—We submitted to analysis eggs of different species of spiders: they contain albumen, fatty bodies and a large amount of a substance precipitated by water. Ant's eggs gave us the same results. These researches are to be continued in the course of the coming season.

Of the eggs of Mollusks.—The analysis of Snails' eggs, which we shall complete next season, seems to show us that the eggs of Mollusks differ entirely in their composition, from those of other animals. Those which we procured, presented no trace of fat; they were made up exclusively of hyaloid membranes containing a viscous colorless liquid. This liquid contains in solution an organic azotized substance, not albumen, for it did not coagulate with heat. It is precipitated by acetic acid and dissolves in hydrochloric acid without any violet color.

Conclusions.—We have shown, in three successive communications composing our memoir, the facts established by our researches on the eggs of different animals, belonging to all the great classes of Ovipara. Let us by way of recapitulation, endeavor to state in some general propositions, the most important consequences which seem to be the results of this first work. We have shown:—

1st. That there exist fundamental differences between the composition of the eggs of animals, and that under this collective name of *egg*, designating the product of the ovarian apparatus intended to contribute to the perpetuity of the species, very diverse bodies are comprised different as possible from one another.

2nd. That among the vertebrated animals, the eggs of birds, of reptiles, and of fish, present in their composition, differences which the simplest analysis cannot mistake, and besides that the eggs of Sauria and Ophidia bear great analogy to those of birds, while the eggs of Batrachia resemble those of the cartilaginous fishes.

3d. That the eggs of Arachnidæ and insects differ altogether, as to their composition, from the eggs of other animals.

4th. That those of Crustacea, organized for living in water, do not at all resemble those of fish or of other amphibious vertebrata.

5th. That this extends to the eggs of Mollusks.

6th. That these differences correspond not only to classes or orders; that they extend to natural families even, without stopping there, since we have proved that an egg of a cartilaginous fish has not the same composition with that of an osseous fish; but further, that a Carp's egg is very different from a Salmon's egg; that the egg of an Ophidian such as an adder's, does not contain the same principles as those of the Chelonia.

7th. That if the composition of different proximate principles is the same in very nearly allied species, the form and the size of vitellin granules vary in a manner sufficiently appreciable to be able to be recognised and assigned to each species.

8th. That the albuminous substances furnished by eggs of birds, reptiles, fish, crustaceans, present in their chemical properties and in their point of coagulation, differences which permit us to suppose that these bodies are made up of different proximate principles.

9th. That an egg changes its nature,—that its liquids alter considerably at different epochs of its formation, when detaching themselves from the ovary, and resting in the oviduct before being hatched.

10th. After having established in the eggs of different animals, the presence of several new proximate principles, ichthin, ichthulin, ichthidin, emydin, and comparing these results with those which MM. Dumas and Cahours obtained in the analysis of hens' eggs, we do not hesitate to propose to science to admit the existence in eggs of a new class of organic bodies, comprising some proximate principles which we will hereafter designate under the name of Vitelline substances or Vitelline bodies.

ART. VIII.—*Observations on the Extent of the Gold Region of California and Oregon, with notices of Mineral localities in California, and of some remarkable specimens of Crystalline Gold;* by WM. P. BLAKE, Geologist of the U. S. Pacific R. R. Survey in California.

THE information contained in the following brief notes was partly obtained in connection with the government exploration in California for a practicable route for a railroad to the Pacific, and partly during a visit to the mining region and a residence of several months in San Francisco. The localities and minerals found in the course of the explorations for the government will receive a more extended notice in the report now in preparation.

GOLD.—It is not yet possible to state the boundaries of the great gold-field of California. It has been gradually expanding on the north, west and east with the progress of exploration, until placers have been worked under the snows of the high ridges of the Sierra, and it appears probable that the crest of that great mountain chain is overlaid by the precious dust. On the north and west, new placers of unequalled richness are constantly discovered, and gold is brought from nearly all of the numerous ranges and ridges that trend off from the upper or northern portion of the Sierra Nevada, and traverse nearly the whole breadth

of the State and a portion of Oregon, reaching the coast between Cape Mendocino and the Umpqua river in lat. 43° 45′.* The placers are therefore no longer confined to the State of California but extend into Oregon, not only to the Umpqua river but beyond it, throughout both Oregon and Washington Territories to the parallel of 49°.† Of this northern portion of the gold region there is however but little known, and the latitude of the Umpqua river may be regarded as the northern limit of general mining operations for the present. On the south, the limits of the field have been extended nearly to the Tejon pass at the head of the Tulare valley in lat. 35.° This point is about forty miles south of Kern river where, according to the recent intelligence the placers are rich and are exciting considerable attention. This river rises in Walker's pass (lat. 35° 39′). and flows westward over a broad area of granitic rocks to the Tulare valley, where it empties into the most southern of the Tulare lakes. South of the head-waters of this river the crest of the Sierra Nevada gradually deflects to the west and the breadth of the exposure of granitic rocks decreases, until at the Tejon, the slopes of the Great Basin and the Tulare valley are only thirteen miles distant. The auriferous slates, (talcose slates,) are not found in the section at the Tejon pass, and this may be considered as the southern limit of the Sierra Nevada gold field.

It is more difficult to determine even approximately the eastern and western boundaries of the auriferous area. The elevated portions of the Sierra having been but slightly explored, its eastern limits are not yet defined. Its western margin along the Sacramento and San Joaquin is better known, but is exceedingly irregular. The greatest expansion of the field from east to west is probably in the northwestern part of the State where gold is found from Mt Shasta to Gold Bluffs on the coast south of the Klamath (lat. 41° 30′)—a distance of 110 miles.

Farther south on the Yuba river the breadth in the direction of the general course of the river is not less than fifty miles. This is believed to be its average width for a long distance southward or to the vicinity of the San Joaquin, beyond which, its limits become more and more contracted to its final termination at the end of the Sierra Nevada. The average breadth of the field for its entire length may be said to be not less than fifty miles.

* It is interesting to observe in this connection that when Prof. J. D. Dana, the geologist of the U. S. Expl Exp., passed rapidly over the section of country in 1841, he noticed that the rocks gave indications of the presence of gold and in an article published in this Journal in 1849, he mentions the region as follows: "It is quite probable from indications observed by the writer that gold may be found in many parts of the range of country between the Umpqua and Sacramento,"—Am. Jour. Sci., [2] xii. 262.

† This statement is made on the authority (verbal communication) of Dr. John Evans, geologist of Oregon and Washington Territories.

If, for the convenience of making an approximate estimate of the area of that portion of the auriferous belt that is now partly explored and worked, we assume the latitude of the mouth of the Umpqua river (43° 45′) as the northern margin, we find that placers are worked over nearly nine degrees of latitude; or if the distance be measured in the direction of the trend of the field, the region is over 700 miles in length. Multiplying this length by the average breadth (50 miles), we have as the result, an area of 35,000 square miles. If, instead of the parallel of 43° 45′ the parallel of 49° is regarded as the northern limit of the gold region, it extends over fourteen degrees of latitude or over about 1100 miles. According to this estimate a very considerable portion of the field lies to the northward of the state line of California (lat. 42°) and is within Oregon and Washington Territories. There is evidently no natural separation or boundary in the auriferous region of California and Oregon, and it must therefore be regarded as one continuous field.

It is desirable for convenience of description and reference that the whole gold region should be known by a suitable name, and that it should be separated into convenient geographical divisions.* Our knowledge, however, of the region north of the California line (or north of the Umpqua) is yet so limited that it is useless to propose any division beyond the general one of *Oregon Mines* and *Washington Mines*, which will naturally be adopted as explorations extend over those Territories.

In California the gold district extends through, and in some cases includes the following Counties: Klamath, Trinity, Humboldt, Siskyon, Shasta, Bute, Sierra, Yuba, Nevada, Placer, El Dorado, Sacramento, Calaveras, Tuolumne, Mariposa and Tulare. The counties of Tuolumne, Mariposa, and Tulare are each large enough to form several of the size of Placer and El Dorado.

It is already the custom in California to speak of the mining region south of Stockton as the *Southern mines:* this name passed into general use before the placers in the northwestern corner of the State were discovered. In 1854 a map of the *Northern* and *Middle mines* was published in San Francisco, but no boundaries for these divisions were given.

The division of the California district into three great and nearly equal areas with the above appellations, is certainly desirable, and an excellent natural boundary between the northern and middle divisions is found in the upper Sacramento and Pitt rivers; while the Calaveras river is perhaps the best that can be

* It is desirable that a name for the gold field should conform to that which may be given to the great mountain chain of which the Sierra Nevada forms but a part. The writer is engaged in preparing a description of the mountain chains of California, with a view to their classification and nomenclature. The appellation *Columbian Chain* has been suggested as a suitable one for the great line of elevation referred to.

selected as the dividing line between the middle and southern. I therefore suggest that these names be adopted for the areas within the boundaries that I have given.

The Northern mines and those in Oregon are now very productive and important: the gold is considered to be superior in its quality and generally commands a high price among the purchasers. The facilities for access and transportation to them from the Sacramento valley and from the coast, are better than in the Southern mines. A large portion of the supplies is sent to the interior towns from Crescent City and other ports on the coast. Mining operations are conducted at various points on the beach, from famous Gold Bluffs—about 30 miles south of Crescent City—to the Umpqua river. The most important localities are in the vicinity of Port Orford between Gold river (Rogue river) and Coose bay, a distance of 80 miles. Gold is found over the whole distance in the beach-sand from the surface to a depth of six feet or more; it is in very small and thin scales and separates from the black sand with difficulty. Platinum and the associate metals, iridosmine, &c., are found with this gold in large quantity; and as they cannot be separated from the gold by washing, its value in the market is considerably lessened—indeed it is sometimes difficult to make a sale of the mixed metals in San Francisco.

The black sand is found in enormous quantity, it is very deep, and is irregularly stratified by the tides. It is undoubtedly stirred to a considerable depth by the surf during storms, and this is shown to be the case by the fact that the richness of claims that have been worked is renewed during high tides or a storm.

Placers of San Fernando and San Francisquito—Santa Barbara? Co.—This locality of gold has hitherto received but little attention, although it was known to the Californians long before the gold of the Sacramento valley was discovered. These placers are on the southern flank of the mountains that have a nearly east and west trend from Point Conception to San Bernardino and from the southern boundary of the Great Basin and the Tulare valley.

These placers are about fifty miles southeast of the Tejon pass and eighty south of the Kern river placers; they were worked near the ranch of San Francisquito by Mexicans in 1840, and were abandoned when the reports of the great discoveries at the north reached them in 1849.

Talcose slates apparently auriferous and resembling those of North Carolina, occur in the pass of San Francisquito and are traversed by quartz veins. It is reported that veins of auriferous quartz in that vicinity were worked simultaneously with the placers.

An occasional excitement is produced by glowing reports from this locality, and according to recent accounts new placers have been found in the vicinity of Los Angeles. Although we are not yet aware of the extent of these placers, it may be safely asserted that they will not compare favorably in area or richness with those of the Sierra Nevada. They are comparatively local in their extent, but the region is worthy of a careful examination.

Gold of the Great Basin—Tulare Co.—Armagosa Mines.—A vein of auriferous quartz traverses one of the granite ridges of the Great Basin near the Mormon trail to Salt Lake and about 170 miles from Los Angeles. The vein has been prospected and attempts to work it have been made by several companies organized in San Francisco, but it is now abandoned.

The gold is found in wire-like filaments ramifying through quartz and carbonate of lime. I have an interesting specimen in which a string of gold traverses a rhombohedron of carbonate of lime and protrudes from its opposite faces. The form was reduced to a rhombohedron by cleavage.

The occurrence of gold in place, in one of the ridges of the Great Basin, so far removed from the Sierra Nevada gold field, is an important fact, and renders it more than probable that extensive placers will be found throughout its length and breadth.

Colorado River—San Diego Co.—It has been frequently reported that gold exists along the Colorado river not far from Camp Yuma at the mouth of the Gila. I could not obtain satisfactory evidence of the truth of this statement; but if it does occur, it is far from water and vegetation and prospectors are obliged to carry the earth they wish to test many miles before water enough to wash it out can be found.

On the western slope of the mountains between San Diego and the desert, there are good indications of gold at several points west of Santa Isabel and near the travelled road. The region is worthy the attention of prospectors.

Coast Mountains, Santa Cruz Range.—According to J. B. Trask,* gold has been found in the Coast mountains, in the counties of Monterey, Santa Clara and San Luis Obispo.

AURIFEROUS QUARTZ.—Quartz veins are found in great numbers traversing the slates, the granite and greenstone rocks of various portions of the Sierra Nevada gold field; but comparatively few of them have been worked to any extent. Among those that produce the most interesting specimens, the following may be mentioned.

Nevada Co., Lafayette and Helvetia Mine.—Beautiful plates and angular masses of gold are found imbedded in snow-white quartz in this mine. They are frequently intimately associated

* Report on the Geology of the Coast Mountains. Sacramento. Senate Doc., No. 9. Sess. of 1854. p. 58.

with brilliant crystals of white iron pyrites, and in other specimens the gold is entirely isolated from the sulphurets and is surrounded on all sides by the compact opaque quartz. Specimens of this character are sought after by jewellers and are cut and polished for ornamental purposes. An immense quantity of "quartz-gold" is ground and polished in San Francisco into shapes suitable for rings, bracelets, cane-heads and the like. Specimens for this purpose are mostly obtained from the placers, but some of the purest and most brilliant are procured from this mine.

Nevada Co., Grass Valley—Gold Hill Mine.—Extraordinary specimens of gold in large smooth plates have been found in one of the veins of this mine. They traverse a semi-crystalline quartz, and in some specimens the gold is crystalline and is implanted among the ends of the quartz crystals.

Specimens that I obtained, have many interesting mineralogical peculiarities and appear to throw light upon the phenomena of the deposition of the metal.

Nevada Co., Grass Valley—Ophir Hill.—The Empire Company are working a quartz vein at this place, which bears a large amount of white iron pyrites, remarkable for the quantity of gold it contains. If a specimen of the pyrites which does not appear to contain gold, is placed in strong nitric acid so that the sulphuret is partly dissolved, gold becomes visible in many points, showing that it exists in a form favorable for collection.

Tuolumne Co., Marble Springs (Merced River).—An interesting quartz vein at this locality bears plates of gold, iron pyrites, galena and zinc blende. These mineral are abundantly disseminated in a compact gangue of white quartz and form beautiful specimens for cabinets.

Placer Co., Volcanoville.—This place is opposite Forest Hill on the middle Fork of the American river, and is noted as the locality of one or more very rich quartz veins. They occur traversing slates in connection with erupted rocks and contain iron pyrites and free gold, disseminated in irregular masses. Specimens of unusual value and weight have been broken out of parts of one of the veins, and gold can now be found ramifying through the quartz where it has been uncovered and brought to view by the removal of the earth from the top of the vein.

Placer Co., Georgetown—Mamaluke Hill.—A quartz vein of unusual richness occurs at this place traversing the talcose slates and apparently conforms to the bedding. It is narrow and much decomposed, the quartz being cellular and friable, and is highly charged with irregular filaments and ragged masses of gold. The decomposition and discoloration of the vein and the adjoining slates rendered it so obscure that it remained unnoticed while the claim was worked as a placer mine. I had the satisfaction of pointing out its true character and washing out the first prospect,

which amounted to several dollars. Within a few weeks after this discovery, the owners of the claim were taking out 274 ounces a week, sometimes finding four ounces in a pan. The last accounts that I have received state, that sixty-five thousand dollars worth of gold had been obtained—all from an ordinary mining shaft fifty feet deep, and without the aid of machinery.

CRYSTALLINE GOLD.—Good crystallizations of gold are comparatively rare in California. A hundred pounds of coarse placer gold may be carefully examined without finding a single well formed crystal. This may partly result from the custom among the miners of reserving any peculiar or remarkable specimens of small size for breast-pins or for preservation as curiosities.

Placer Co., Forest Hill.—Interesting octahedral crystals have been found in the claims of the Messrs. Deidesheimer at this place. These crystals occur with placer gold 2500 feet above the level of the river: but they are not much worn by transportation. Quartz crystals are found mingled with the auriferous earth with only their sharp edges and angles removed by attrition, which shows that the drift is comparatively local and indicates the presence of a parent vein in the vicinity. Most of the crystals have the peculiar triangular depressions in the faces, generally found in gold crystals, and some of them are very much distorted; others are flattened parallel to a face so as to become thin triangular plates. These specimens are seldom more than three-eighths of an inch across the base.

An imperfect octahedral crystal of extraordinary size was taken from the claim last year. The planes are only partially developed for a short distance above and below the basal ridges, and the peculiarity of a series of similar parallel planes, lying like plates one within the other, is presented.

The crystal is elongated in the direction of a line parallel with two basal edges and thus becomes a rectangular octahedron.

The length of the longer base is one inch, and the shorter seven-eighths of an inch. I believe this to be the largest crystal ever reported; it may be called a *skeleton* crystal on a grand scale.

Arborescent and Dendritic Gold—Placer Co.—Some of the most remarkable and beautiful specimens of gold ever seen, have been found at Irish creek three miles from Coloma. They simulate the veined and reticulated appearance of leaves and more closely resemble the foliage of the Arbor Vitæ or the fronds of the most delicate ferns than of any other forms of vegetation. The filamentous and arborescent masses are frequently united to plates (as broad as the hand) which are covered with lines of crystallization and are brilliant with numberless faces of partly formed crystals. They are also combined with good crystals which are generally octahedral and have perfect faces.

I have a very beautiful specimen of this character in the form of a leaf: one side is beautifully arborescent, and the other is studded with perfect octahedrons of various sizes and about twenty-five in number, including the smallest. They are geometrically arranged, all their similar edges being parallel. This is believed to be the most remarkable and beautiful specimen known. Its weight is 17 pwt. 10 grains. Length, two and one quarter inches, width, one and a half inches.

One of the foliated specimens in my collection, bears a crystal having the form of a pentagonal dodecahedron with cavernous faces.

One of the largest specimens of this arborescent and foliated gold that has been procured, was about twelve inches long and about four broad. A part of the specimen was a plate three or four inches long, covered with triangular marks; the remainder was arborescent, and the whole appeared to have grown from one end.

Another specimen slightly different in its character and probably from another locality in the vicinity, was ten inches long, three broad and about half an inch thick. It weighed 31 ounces, and was free from quartz; forming a most beautiful mass of a rich yellow color and a delicately marked surface, consisting of a net-work of fibres. It appeared like a bundle of broken fern leaves closely matted together.

These specimens are evidently from a quartz vein, but although I have visited the locality, I have not been able to see the place from which they were taken, or to obtain any reliable information concerning their mode of occurrence and the associate minerals. Some of the foliated specimens were incrusted with a thick scale of sesquioxyd of iron.

The locality is about three miles from Sutter's mill—the point where the gold was first discovered.

Platinum.—The occurrence of this metal and its associates with the gold of Port Orford, has been previously noticed in this Journal.* It appears to constitute a large percentage of some of the samples of gold brought from that region and this renders the locality peculiarly interesting. Several ounces of the mixed metals were obtained, and are now being carefully examined. The platinum is in very small thin scales and is easily lifted by a magnet. The difference in the specific gravity between this metal and gold is beautifully shown by the newly invented separating machine,† which distributes black-sand, gold and platinum in separate zones at the upper edge of the oscillating copper table.

I have found platinum in small quantities in the gold of the Middle mines, and in the fine scale gold from the forks of the American river, but it appears to exist in small quantity. It is

* Second Series, vol. xviii, p. 156.

† Invented by Horatio Bradford, Esqr., of New York.

an interesting fact that the metal is more common in the Northern mines, and that it is most abundant on the coast.

Mercury.—*Santa Clara Co., New Almaden.*—The ore at this mine is a massive sulphuret (cinnabar) and its character and association have already been described in this Journal, vol. vi, p. 270, and xvii, p. 438.

The rocks at the locality appear to be metamorphosed sedimentary strata: They crop out at several places below the mine on the side of the hill, and consist of regular strata of argillaceous shales and layers of flint and jaspery rock, which resemble those of San Francisco near the Mission and Fort Point. Serpentine rock is found in, and near the mine and trappean rocks are also found in the vicinity.

The similarity of these strata and the serpentine to those of San Francisco leads me to consider them as of the same age at each place, and it is probable that the flint and jaspery rock is a metamorphosed portion of the blue sandstone formation of San Francisco.

Guadalupe Mine.—This is another locality of cinnabar about three miles from the New Almaden mine, but it is not now worked.

Monterey Co.—Other localities are reported in this county and at one point a vein has been opened by parties residing in Monterey. I have no definite information of its extent, but specimens of the ore of fair quality were exhibited in Monterey.

Copper.—Small amounts of copper pyrites and green coats of the carbonate resulting from its decomposition, are found in many of the veins of auriferous quartz that have been opened in different parts of the State. I have not, however, yet heard of any locality that promised to reward explorations conducted for the copper alone. According to J. B. Trask, sulphuret of copper and the blue and green carbonates occur with quartz in Monterey Co. at Alisal; also in Santa Barbara Co. and San Luis Obispo.

A vein of copper pyrites occurs on the slope of the Great Basin about seven miles east of Johnson's river. It is associated with quartz and by its decomposition has produced abundant green crusts of malachite. Oxyd of iron in fine powder is found in the cells and cavities of the quartzose gangue, and appears to have resulted from the decomposition of the pyrites. It is very probable that this vein is auriferous, but the specimens have not yet been examined. The vein traverses a micaceous granite and appears (from the outcrop) to have a width of over twenty feet, including the quartzose gangue. This is a valuable vein, and doubtless will show some splendid ore when thoroughly opened. Its discovery is one of the results of the geological reconnoissance in connection with the R. R. Survey.

Copper pyrites is also found in a vein about seven miles below the summit level of the New Pass, which leads from the Great Basin to the valley of San Francisquito. This vein outcrops on the southern slope of a granite hill on the north side of the Pass, and is about 90 feet above the bed of the creek. The ore resembles in its luster and color the variegated copper pyrites, but is much softer. It is found in strings and narrow veins distributed through a hard quartzose gangue about fifteen feet thick: the thickest seam of ore, however, does not exceed two inches; but when several such were closely combined a thickness of eight inches of good ore was seen.

This vein has been prospected and a small quantity of ore broken out. It is about sixty miles distant from Los Angeles by the trail.

Native Copper and Red Oxyd of Copper.—When visiting Camp Yuma at the junction of the Colorado and the Gila rivers in Dec., 1853, several large masses of superior copper ore were shown to me by the officers of the fort. This ore was brought from the adjoining State of Sonora, Mexico, and the vein is reported to be near Altar. It is within the limits of the strip of territory recently acquired by purchase and is therefore now in the United States. Specimens are frequently brought in by emigrants who cross the Colorado at the ferry below the fort. The ore is principally the red oxyd of copper associated with the pure metal and green crusts of carbonate. The specimens that I saw ought to yield about ninety p. c. of pure copper. This is probably the ore that has recently excited so much attention in California, and has been reported to be highly charged with gold.

Calaveras Co.—Native Copper and Silver.—A specimen of pure copper combined with silver is reported to have been found in a placer mine near Mokelumne Hill. The specimen was exhibited in several places and sent to San Francisco, but I have not been able to obtain any reliable information concerning it, or the circumstances under which it was found.

Iron ores.—*Mariposa Co., Burn's Creek.—Limonite.*—An outcrop of hydrous sesquioxyd of iron or limonite occurs near the banks of this creek, on the right of the road going south. It is associated with a quartz vein and forms a bed about twenty-five feet thick, lying conformably with talcose and chloritic slates. The ore outcrops in great solid blocks from two to four feet in diameter: it is compact, of a dark brown color, and breaks with a smooth conchoidal fracture. The position and peculiarities of this ore indicate that it has resulted from the decomposition of pyrites, and that it forms the "gossan" of a vein of sulphuret below the surface. The mass does not, however, present that cavernous and friable condition in which gossan is generally found.

Magnetic Iron Ore.—A massive and fine-grained variety of this ore is found associated with one of the auriferous quartz veins of the county. Specimens that I have seen were colored green by their layers of carbonate of copper. It has polarity and lifts small fragments.

Tulare Co.—Magnetic Iron Ore.—This ore occurs in a bed or vein about three feet thick in a low ridge of white crystalline limestone at the summit of the pass known as the Cañada de las Uvas. The ore is compact but not crystalline, when broken it shows a brilliant fracture and a granular surface, and does not break with flat faces, like the massive magnetic ores of New York and New Jersey.

New Pass—Magnetic Iron Ore.—Specimens of very pure and highly crystalline magnetite were picked up in the valley of this Pass: it is associated with hornblende, cinnamon-colored garnets and chlorite.

Placer Co., Volcanoville.—Large boulders of compact magnetic ore occur in the bed of the creek that flows by the side of the great vein of auriferous quartz at that place. These boulders are so large and abundant that it is probable a vein of ore will be found in sites in that vicinity.

San Francisco Co.—The fissures and crevices in the serpentine rocks of San Francisco are occasionally drusy, with small but brilliant octahedral crystals of magnetite. They did not give reactions for chromium when examined before the blowpipe.

Sulphuret of Iron.—Good crystals of pyrites are obtained in the talcose slates in various parts of the mining region. At Georgetown, Placer Co., it is abundant in minute cubic crystals. They are obtained free from rock or gangue as one of the products of gold washing, and as they are very brilliant and of uniform size they are worthy of a place in good collections.

Chromic iron.—*Monterey Co.*—Massive chrome ore of excellent quality was shown me in San Francisco and reported to be from a short distance south of the Mission of San Juan. It is an interesting fact that it is almost identical in its appearance with the ore from "Wood's pit" in Maryland and like it, is partly covered with green coats and crusts of emerald nickel. The extensive distribution of this mineral in California has been noticed at length by Mr. P. T. Tyson in his report.*

Antimony.—*Tulare Co.*—A large vein of the sulphuret of antimony, (antimony glance,) exists in the high granitic range that borders the Tulare valley on the south. It is about eighty miles from Los Angeles and is most readily visited from the Tejon. By observations with the barometer, I found the outcrop of this vein to be at an altitude of about 6000 feet above the sea. It is on the side of a precipitous ridge of granite and not favorably situated for examination. Its thickness was estimated to be ten feet

* Ex. Doc., No. 47. 31st Cong., 1st Session. (Senate.)

or more. A steep chasm or channel extends from the top of the ridge to its base, and is partially filled with rocks and the debris of the vein. Solid blocks of the ore were found with this accumulation, having been broken out from the vein above; one of them was twenty-seven inches long and sixteen to eighteen wide.

The ore is associated with quartz, and where it has decomposed, an abundance of antimony ochre is found, together with crystals of selenite. Specimens of quartz traversed by long prismatic crystals of the ore were obtained.*

Salt.—Salt is found in small quantity as an incrustation or efflorescence on the soil along streams or on the margins of ponds in nearly all parts of California. It appears to be most abundant in connection with the tertiary strata and in the streams that flow from them. It is doubtless the fact that a great part of the incrustations called soda, consist principally of common salt.

Tulare Co.—Cañada de las Uvas.—There is a small shallow lake near the central part of this Pass fed by springs and streams from the adjoining valleys and ridges which are partly of tertiary strata. During the summer season the water of this lake evaporates, and its bed becomes covered with a white crust of salt which glitters in the sunlight like a field of snow.

Taheechaypah Pass.—A lake of a similar character to the one just described is found in one of the elevated valleys of the Sierra Nevada near this Pass. At another locality in that vicinity and near the margin of the Great Basin, salt occurs in a thick bed, from which over one hundred mule-loads have been taken, and carried to the Tejon Indian reservation for the use of the Indians.

This salt is perfctly white and amorphous, being reduced to a fine powder by simple pressure. It is sufficiently pure for table use.

Dry salt lakes are also found near the termination of the Mojave river in the Great Basin, and at many other places throughout Southern California.

Los Angeles Co.—Salt is now manufactured in large quantity from sea-water by solar evaporation on the coast near Los Angeles.

Lower California.—A dry salt-lake has been discovered about 250 miles south of San Diego and near Marguerita bay. It forms a thick bed and is very pure, being well crystallized in large hopper-shaped crystals. It is reported that the locality has been purchased by capitalists and that the salt is being shipped from there in large quantity.

Gypsum.—Transparent plates of selenite are common in the soft unconsolidated tertiary strata in various parts of the state. At some localities it forms seams or beds several inches thick lying conformably with the stratification. In Tulare Co., at Ocoya

* A more detailed notice of this locality will be found in the author's Preliminary Geological Report accompanying the Report of a Reconnoissance and Survey in California, by Lieut. R. S. Williamson. House Doc., 129. 1855.

creek these transparent plates are found in the Miocene strata; some of them are combined with the fibrous variety, and form beautiful cabinet specimens. Good crystals are also found in this county, at the antimony localities. Thin transparent plates are numerous in the Miocene strata bordering the Colorado Desert, and on the borders of Carrizo creek they are found lying loose upon the surface where the strata have been worn away by the rains. Seams of gypsum are numerous in the tertiary strata of Benicia.

Wherever I saw the gypsum in the tertiary strata, it appeared to have been formed by the infiltration of sulphates and their decomposition by the lime of the beds.

Bitumen.—The occurrence of bitumen springs in the Coast Mountains has been noticed in the writer's preliminary Report.*

Near the Pueblo de los Angeles, there is a large pond or lake of the bitumen about one quarter of a mile in diameter. Its central portion is soft and semifluid but the outer parts are hardened by exposure. The material is much used for covering roofs, and at Monterey I saw a good basement formed from its mixture with sand and gravel.

Sulphur.—Very interesting specimens of sulphur can be obtained at the Geysers or hot springs in Napa Valley. It occurs in crusts or lining fissures in the soil and tufaceous deposits around the springs, and is in small crystals, forming drusy surfaces.

Beryl?—*Tuolumne Co.*—Small and well formed hexagonal crystals having the hardness and color of beryl have been obtained from the Tuolumne river three or four miles from Jamestown. The specimens that I saw were apple-green, and one of the smallest was emerald-green and transparent. The largest crystal was nearly $\frac{1}{4}$ of an inch in diameter and terminated at both ends with the planes R, and $-\frac{1}{2}$, as in tourmaline. I was unable to retain the specimens for further examination.

Tourmaline.—*San Diego Co.*—Black tourmalines of unusual size (from six to eight inches in diameter) occur abundantly in the huge feldspathic veins that traverse the granite ridges bordering the elevated valley of San Felipe, in the mountains between San Diego and the Colorado desert. These crystals are not perfect.

Feldspar.—Orthoclase.—*San Diego Co.*—Good crystallizations of this mineral can be found in the granite veins near the road between Santa Isabel and San Pasquale. They are associated with tourmalines and garnets.

Andalusite, *Mariposa Co.*—This interesting mineral was found in great abundance in a conglomerate that caps the hills along the Churchillas rivers (San Joaquin valley) at the crossing of the road leading to Fort Miller.

* See also this Journal, [2] xix, p. 433.

Very fine crystals of unusual size occur in the gravel along the bank of the stream. I picked up several that were two inches long and three quarters of an inch in diameter. They have a delicate pink or rose color and some of them are translucent. The peculiar tesselated appearance displayed in a cross section of crystals of this species, is exhibited by these specimens in a beautiful manner.

CALCITE.—Crystallizations of this mineral are found at the Quicksilver mine, (New Almaden), at the Pass of Jacum, San Diego Co, and on the surface of the Colorado desert north of Carrizo creek, where some transparent crystals were picked up. It also occurs in beautiful stalactites and delicate crystals in the great cave in Calaveras Co.

ART. IX.—*Analysis of Idocrase from Ducktown, Polk Co., Tenn.;* by J. W. MALLET, Ph.D.

THE specimen of this Idocrase examined occurred among some other minerals from the Ducktown copper mine, in thin bladed crystals imbedded in a mixture of copper and iron pyrites. The crystals were from half an inch to two inches in length, about half an inch in width, and not more than an eighth of an inch thick. They were longitudinally striated, and presented scarcely any planes, lateral or terminal, suitable for measurement by the reflecting goniometer. The existing planes however seemed to be 0, 1, ∞, 1-∞, and ∞-∞, the last being most developed, and the others very small. The mineral was nearly colorless, exhibiting but a slight tint of brownish or yellowish green, and was subtransparent. H. =6 5; sp. grav. =3·359, taken with fragments.

Analysis gave the following results, a little copper present being calculated as copper pyrites (derived from the gangue), and the necessary amount of iron subtracted from the total quantity of oxyd of iron weighed:

		Atoms.	
Silica,	38·32	·846	
Alumina,	25 68	·500	
Protoxyd of iron,	8·13	·226	1·151
Lime,	25·39	·907	
Magnesia,	·36	·018	
Copper pyrites,	1·91		
	99·79		

We have here a slight excess of alumina over the amount required by the formula 3RO, SiO_3+Al_2O_3, SiO_3, and a slight defect of protoxyds. The fragments of mineral were selected with great care, and contained no visible particles of gangue—hence the copper found in the analysis may perhaps, as in Cyprine, belong to the Idocrase itself, in which case the protoxyds would be increased by tbe copper and iron calculated above as copper pyrites.

ART. X.—*Observations on Binocular Vision;* by Prof. WILLIAM B. ROGERS.

THE remarkable discoveries of Prof. Wheatstone in relation to binocular vision, and the observations of Sir David Brewster, Prof. Dove and others in the same field, must be regarded as among the most important contributions ever made to the science of physiological optics. Nor has the knowledge of these beautiful and curious results been confined to the circle of scientific inquirers. The diffusive intelligence of the age has converted the stereoscope into a popular source of instructive recreation. But much as has been done, especially by Wheatstone and Brewster, towards forming a true theory of binocular vision, it must be apparent to all who are familiar with their labors that these observers differ in some important particulars, as well in their account of the phenomena as in their mode of interpreting them.

In the following pages I propose to record the substance of a series of observations, begun some years ago with the view of repeating and varying the experiments, and if possible testing the conclusions previously made known in regard to this interesting subject. It will be seen that the modes of observation on which I have fallen are in some respects new, and have the advantage of great facility, and that the results and the inferences which I have drawn from them, lead to views of the process of binocular vision differing in some respects from those of either of the eminent philosophers to whom I have referred. To form a theory of this process in all respects satisfactory may require further observations and greater subtlety of metaphysical interpretation, but I trust that the facts and conclusions which I have to present will throw light on some obscure points of the subject and will perhaps disclose features of it which have not hitherto been noticed.

PART FIRST.

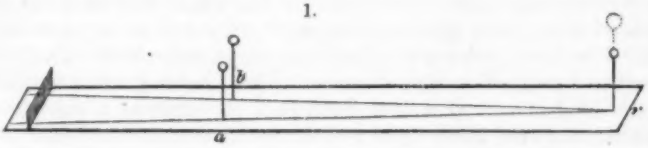
1. *On the place in which we perceive objects that are binocularly combined.*

The learned Dr. Smith of Cambridge, in his "Complete System of Optics," p. 388, describes an experiment in which by binocular vision he united into one long perspective line the legs of a pair of compasses partially opened and held in a particular position directly before him, and he ascribes the apparent union to the fact that the pictures of the corresponding parts of the two legs fall "upon the same points of the retinas as that of the object to which the eyes are directed." It is therefore clear that this writer apprehended the primary Law of binocular combination very much as it is at present understood. Although mistaken in his account of part of the experiments and in other ob-

servations in binocular vision, he is I think entitled to the credit of having first perceived a chief condition of these phenomena.

But the law of the apparent place of objects binocularly combined is so fundamentally important as to call for other and better modes of proof. One such, remarkable for its completeness, has been described by Sir David Brewster (Phil. Mag., 3d Series, Vol. 24), and I may be permitted to add the following simple experiments for the same purpose which I have found very satisfactory and easy of execution.

1.



On a light strip of wood about 3 feet long and 4 inches wide, two lines are to be drawn diverging from one end at such an angle that their terminations at the other end shall be a little nearer one another than the pupils of the two eyes (fig. 1). Three steel pins each about $2\frac{1}{2}$ inches high and surmounted by round heads $\frac{1}{8}$th inch in diameter may then be fixed perpendicularly in the wood, one of them at the angular point, and the two others at points of the lines not nearer than the limit of distinct vision. The instrument is to be brought close to the face, in such position as to enable us at the same time to cover *r* by *a* in the right eye, and by *b* in the left. If now we direct our attention to the remote pin *r* we see what appears to be another pin thicker and taller than *r*, coinciding with it or very near it. This is the binocular resultant of *a* and *b* as seen by the two eyes converged to *r*.

To obtain this effect in the most satisfactory manner the unpractised observer will find it useful to conceal the surface of the board by interposing a low screen between *a b* and the near end of the instrument, or he may effect the object by depressing the remote end until the lower part of *a* and *b* ceases to be visible. If, while directing the eyes steadily towards *r* so as to perceive the resultant image of *a* and *b*, we gently vibrate the instrument in its plane around a point midway in *a b*, we see the remote pin passing across the position of the compound image, sometimes behind and sometimes in front of it, but always very near. This curious result never fails to show itself, if we are careful to direct the eyes steadily upon the image while moving the instrument, and it gives us a clear impression of the place of the resultant as coincident with the point to which the optic axes are converged. It is important to observe that while viewing the compound image in this experiment, we see, on the right of it, the image of

the right pin as visible to the left eye; and on the left of it, that of the left pin as seen by the right eye; and, provided the eyes be steadily fixed on the resultant, these lateral images appear to lie at the same distance and to have the same magnitude as the resultant.

The following variation of this experiment is easily repeated, and illustrates the same law of binocular combination in a very simple and striking manner. Omitting the remote pin and fixing the two others about 18 inches from the near end of the board, and on opposite sides, at an interval not much less than the distance between the centres of the eyes, let the observer place himself 6 or 8 feet from a window commanding the view of a bright wall or a roof covered with snow. Then holding the instrument with its farther end a little depressed, or interposing the screen, let him vary the convergence of the optic axes until he has formed the resultant image, and is able to view it steadily. He will observe that this image seems to occupy a position outside of the window, and he will have the feeling that *he is looking at it through the glass.* Retreating a few steps, he will see it as if situated in the plane of the window, and as he recedes still farther in the same direction, it will appear within the apartment between him and the glass. Throughout these changes he cannot fail to notice that it maintains a constant distance from the eyes, and has for its position the point of meeting of the optic axes.

This effect is even more striking when the bright surface in front is that of a globular astral lamp. In this case by commencing the observation at a distance of 12 or 15 feet from the lamp we see the binocular image on the near side of this object, but as we slowly approach we observe the image apparently *penetrating the luminous globe*, and at a less distance still we see it beyond the lamp as if we were looking through the glass at a real object in that position.

In the preceding cases the resultant is formed by converging the optic axes to a point more remote than the two objects to be united, but it is easy, by a different arrangement of the pins, to prove that the same law holds for the position of the binocular

2.

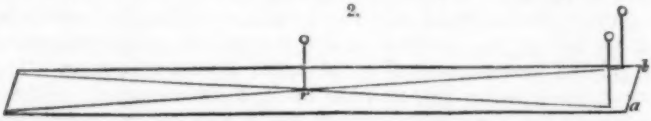


image when the point of convergence is nearer than the objects. For this purpose *a* and *b* are to be fixed at the further end of the board, and *r* at some intermediate point in the central line (fig. 2). Then directing the optic axes steadily towards *r* we see the

compound image placed between the two lateral ones at or very near *r*. By giving the board a slight vibratory motion around the end *a b* we cause the pin *r* to pass alternately to the right and left at a small distance behind or before the image, while the latter is seen to maintain its position at the point of intersection of the optic axes. By making *r* the centre of the vibration we produce a like movement of *r* in regard to the image.

In either of the modes of adjustment which have been described this instrument may be used as a stereoscope. It is true that those observers, who by any practise have acquired the power of converging their optic axes steadily upon near and remote points at will, do not require the aid of a fixed point towards which to direct them, and can readily without any instrument combine the corresponding pictures and obtain the full stereoscopic effect. But most persons find great difficulty in maintaining the necessary adjustment, and need the assistance of some guide-object as the pin *r* in a proper position.

By placing stereoscopic drawings of suitable dimensions in an upright position against the pins *a b* (fig. 1), and directing the eyes to or beyond the remote pin, which in this case should be taller than the others, we see the resultant perspective image at the place to which the axes are converged; or if we rest the drawings against *a b* (fig. 2), and converge the eyes to *r* or to some point readily found, before or behind it, we see the resultant image perfectly formed in that position. It is hardly necessary to say that, in the former case, the points of the twin figures which are to be united, should be at a less distance apart than that which separates the centres of the eyes. When the distance is exactly equal to *a b* (fig. 1), the image will be formed at or near *r*. In the second case this restriction as to the space between the drawings does not apply. When the interval of the corresponding points is equal to *a b* (fig. 2), the resultant picture will be formed at *r*; when the interval is greater, *r* must be moved nearer the eyes; when less it must be moved toward *a b*.

2. *Of the apparent distance and magnitude of the lateral or component figures.*

In all experiments in which the component figures are seen along with the resultant image, an interesting fact is observed in regard to their size and distance from the eye. When first formed the resultant appears to occupy a position in advance of or behind the plane of the compound figures, according as the axes are converged to a point nearer than that plane or beyond it; and as long as the attention is divided between the resultant and the component figures, this relative position is maintained. At the same time, the resultant appears smaller in the former case and larger in the latter than the components. But if we fix the eyes

steadily on the resultant, we see the two other images take their places beside it in the same plane and of the same magnitude as its principal section if it represent a solid.

In making this experiment, it is best to use identical figures, such as two equal bodies about an inch in diameter whose centres are 1½ inches apart. If, while the three figures are thus visible in the same plane, the eyes be suffered to glance sideways for a moment, the component figures retreat or advance from the plane of the resultant, and a renewed effort is necessary to bring them back to it. At first some difficulty is experienced in retaining them in this position, as the observer without knowing it is prompted to glance alternately at the resultant and the components and is thereby led to refer the latter to the distance at which they are actually placed. After some practice, however, it becomes easy, at will, to shift the components into or from the plane of the resultant, and at the same time to observe the change of their apparent magnitude.

A similar phenomena presents itself in the case of the two images of a single object seen on directing our attention to a point beyond it or between it and the eyes. When for example we look steadily at the middle of the bright globe of an astral lamp while holding a wire or slender pencil between it and the eyes, the two images of the pencil appear to touch the globe. If however we fix our gaze steadily on the pencil we see two images of the globe, one on each side of it, and at the same distance from us as the pencil. In the former case the pencil appears larger and in the latter the globe smaller than when viewed in their real position.

It would seem therefore to be a law of vision, that, while the eyes are intently regarding a particular object, all others seen at the same time are referred to the same distance, or, in other words, that *the sense of distance impressed upon our minds by the object to which our eyes are specially directed always connects itself with the other objects seen at the same time.*

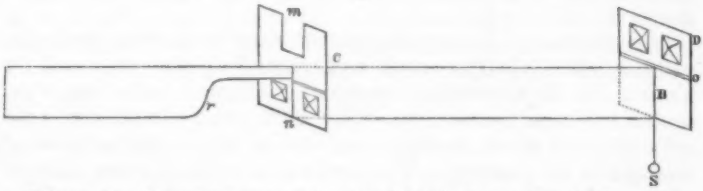
In this experiment it is important to exclude all extraneous conditions which are likely to suggest the true places of the objects. Hence the pencil or rod instead of being held in the hand should be supported in some other way, and a screen of black pasteboard or the dark cover of a book should be interposed, in such position as to hide from view the stem of the lamp and the lower end of the pencil, or whatever may suggest their mode of connection with objects near them. For want of this precaution, the learned Dr. Smith and after him most writers on optics have failed to recognise the law above stated ; for he says (p. 48 Complete System of Optics), "it is manifest that the two apparent places of the object are neither of them the same as its real place, and that they lie *between it and the mark we look at* but not very far from the real place."

As the lateral images tend to withdraw the eyes from the compound one situated between them, and thus to distract the observer's attention, it is necessary, for the most satisfactory results, to exclude these from the view. In the simple form of stereoscope about to be described, this is effected in such a manner that by suitable adjustments we may obtain the resultant image either at a greater or less distance from the eyes than that of the drawings from which it is formed. It will be seen that in general plan this apparatus bears some resemblance to the phantascope of Prof. Locke. The latter however is only adapted to the formation of a compound image between the drawings and the eyes, and it does not exclude the lateral pictures.

3. *Description of the sliding stage Stereoscope.*

This instrument is represented in fig. 3 where AB is a thin strip of light hard wood about 3 ft. long and from 2½ to 3 inches broad to which are attached two square pieces C and D of the same material each about 6 inches square, fitting tightly upon the strip by slits at B and *n* so as to be adjustible at various distances from A. In C is cut a rectangular opening *m* about 1¼ inches wide and 2 inches long. A narrow slip is fixed on the stage D at *o* and another on C at *p* for the purpose of supporting the diagrams when the instrument is held in a slightly inclined position with the end B a little downwards.

3.



When the diagrams are to be combined at a point farther from the eyes than their actual distance they must be placed at *n* on the stage C and the instrument turned around so as to bring *n* above *m*. Should the distance between their corresponding points be much less than the interval separating the two eyes, the union will be readily effected by bringing A close to the face, midway between the eyes, and looking past the diagrams towards the moveable pin S or to some point between it and *n*, which in the rapid adjustments of the axes is almost instantly found. In this use of the instrument the part A*r* of the central strip conceals both the lateral images, the right eye being permitted to see only the right hand figure and the left eye that on the left hand, and these by a due convergence of the axes are united in a single stereoscopic resultant. When the component diagrams are farther apart or when the stage itself is moved to a greater distance from A, the point of convergence proper to their union is

more remote than S. But in this case the union is easily effected by directing the eyes past *n* to a point in the wall or other distant object in the room, and approaching or receding until a suitable convergence is attained. After a little practice even this becomes unnecessary, as we can then at will direct the eyes as if gazing upon objects at various distances behind S.

In this mode of combination, the picture of the object proper to the right eye is viewed by that eye and the picture proper to the left eye by the left, just as with the common stereoscopes. Hence the drawings intended for the latter instruments may be used in this, provided they are so close to one another as to bring the points which are to be united, nearer together than the centres of the eyes. The ordinary twin-figures on black paper can be adapted to very easy combination by dividing the paper transversely so as to separate the figures and then placing them on the stage at *n* in closer juxtaposition. For the greatest facility of observation they should be brought near enough to admit of being combined by converging the axes to S, or to some point between this and the stage C. This stage is represented in the diagram at the greatest distance from A at which a double drawing on the same paper can be used, but it can be moved up nearly to *r* without disturbing the paper, as in the interval the strip is cut away to a narrow stem. When separate drawings are employed they may be moved with the stage to any desired position between B and A.

The absence of the screen between *r* and *n* enables each eye to see a little into the space commanded by the other, and by moving the stage towards *r* we can so adjust it as to make the central line at *n* the inner limit for both eyes. In this way we have it in our power *to combine figures having a line or point of intersection in common.* This, which is a peculiar adaptability of the present instrument, leads as we shall hereafter see, to some curious stereoscopic effects.

When we propose to form the resultant figure somewhere between the drawings and our eyes, it is necessary to place the twin picture or the separate drawings on the stage D, and to adjust the stage C to such a position that looking through the opening *m* with the right eye we see only the left hand drawing, and looking with the left eye we see only the right hand drawing. If both eyes be then directed toward the opening, we observe in or near it, hanging in the air, the beautifully clear binocular resultant.

The resultant figures thus formed has a relief just the reverse of what it would be with the same drawings placed on the upper stage of the instrument, for in the present instance the right eye views the drawing proper to the left eye, and the left that proper to the right. Hence by simply shifting the paper from one stage

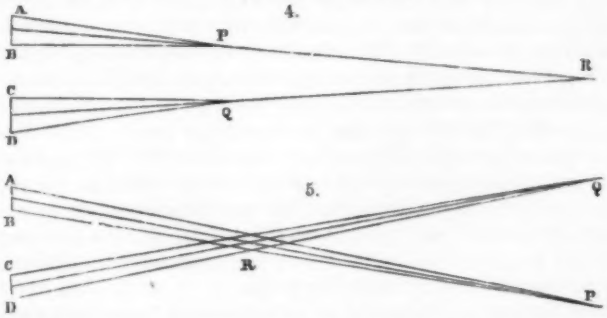
to the other we have the opportunity of alternating these stereoscopic effects.

In these experiments the apparent magnitude of the resultant exceeds that of the drawings when the upper stage of the instrument is used, and falls short of it when the drawings are placed on the lower stage, the change in both cases being determined by the relative distance of the resultant and the drawings from the eyes.

4. *Of the unusual adaptation of convergence and refractive power by which we see the resultant image.*

A feature of great interest in the preceding experiments is the clearness and precision of the resultant image. This is equally true whether the image be formed between the eyes and the diagram or at a greater distance than the latter. In the one case the lines are more slender and in the other case broader than in the drawings, but this is only a part of the general diminishing or enlarging influence due to the relative distance of the resultant and the drawings from the eyes, and does not affect the distinctness of the combined image.

In order that each point of the resulting figure may be thus clearly seen, the eyes must be in a condition suited to the accurate convergence of the rays falling upon them from the corresponding points of the drawings; that is, *they must for the time be adjusted in refractive power to the distance of the diagrams and not to the distance at which the binocular resultant is seen.* At the same time the convergence of the optic axes to a point nearer or more remote than the pictures has the effect of determining our perception of the combined image at that point.



To illustrate this more clearly let P and Q (figs. 4 and 5) represent any two points of the drawings which are seen united at R, and let PAB, QCD be the conical pencils proceeding from them respectively to the left and right eyes. As the resulting image is clear and without blur we are to infer that these conical pencils

are converged to an accurate focus on the sentient surface of each eye at the inner extremity of the optical axis. But that this may be so, the eyes must be adapted in refractive power to the divergency of the pencils or in other words adjusted to the distance of P and Q the actual sources of the rays, although from the convergence of the axes to R we are led to refer the combined image of these points to that position in space.

In these conditions therefore it appears that the act of vision is attended by a peculiar and unusual relation of the optical adjustments. For while in the ordinary mode of viewing an object with the two eyes we adjust both the refractive power and the axial convergence to the same distance, viz, that of the particular point on which for the moment we fix our attention, *in these binocular combinations we are required to adapt the refractive power to the actual distance of the drawings at the same time that we converge the axes to the point behind or in front of them in which the resultant figure appears.* This capacity of the eyes to depart from the usual association of adjustments and to accommodate themselves to the distinct vision of an object in a position nearer or more remote than its true place, has been considered by Prof. Wheatstone in connexion with a peculiar class of experiments with his stereoscope (Phil. Mag., 4 series, vol. iii), but it has not received from writers on physiological optics the attention to which, from its relations to constrained binocular vision in general, it would seem to be entitled.

The mode of union of the two adjustments of refractive power and axial convergence which has been habitual from infancy cannot be interrupted without more or less of effort, and this in the case of persons unpracticed in the experiments above described produces the sensation of a forced condition of the eyes, along with a fluctuating and often confused impression of the binocular image. That these effects are not due to any difficulty attending the axial or refractive adjustments separately considered, is obvious from the fact that we are able with perfect ease to vary the convergency to nearly all distances, and to adjust the refractive power of either eye alone or of both to any point not within the limit of distinct vision. It is therefore to the suspension of our usual association of optical adjustments that I would ascribe the peculiar sense of constraint which at first accompanies the above mentioned experiments in binocular vision.

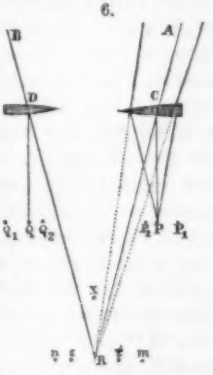
This feeling of constraint so embarrassing to an inexperienced observer ceases after we have learned by a little practice to separate the two optical adjustments as above mentioned. We then find no difficulty in combining the drawings into a precise and clear resultant, and fixing the attention upon it as steadily as upon an actual body in the same position.

5. Of the unusual adjustment of refraction and axial convergence with Brewster's and Wheatstone's Stereoscopes.

The sense of unnatural tension above referred to is not confined to observations with forms of Stereoscope like that just described. It more or less attends the use of all stereoscopes however perfect their construction, or accurate the disposition of the drawings. It may indeed be stated as a general proposition that whenever a perspective resultant is formed by the binocular vision of plane drawings, there are certain parts of the resultant which cannot be seen without a separate adjustment of the refractive and the axial convergence of the eyes. As this condition attending vision with ordinary stereoscopes even in the best arrangement of the drawings, appears not to have attracted the notice of writers on the subject, the following illustrations of it may prove interesting.

In fig. 6 are represented the essential parts of the refracting stereoscope of Brewster now so familiarly known in this country and Europe. P and Q denote corresponding points of the drawings which by the use of the semi-lenses C and D are to be united into one. When these points are properly adjusted as to their distances from C and D and from each other, this union is effected by ordinary unconstrained vision. In this case the conical pencils proceeding from P and Q have their virtual foci coincident at some point R, and the eyes at A and B receive the light exactly as if a real radiant were situated there. But in a different adjustment this coincidence does not take place. Thus if P and Q be moved farther from each other to P_1 and Q_1, the virtual focus of the former will be shifted from R to *m* and that of the latter from R to *n*. If they be moved nearer together as for example to P_2 and Q_2 their foci will be transferred to *s* and *t*. In the former case *m* is the object proper to the eye at A, and *n* that to the eye at B; in the latter, S is the object proper to A and *t* that to B. Hence while the visual lines for *m* and *n* meet when prolonged at a point V beyond R, those for *s* and *t* intersect one another at a point X nearer than R.

6.

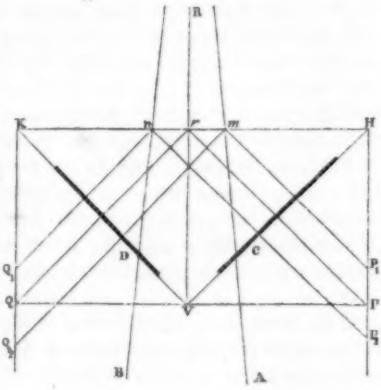


In either of these cases, to combine the two images into a single resultant will require the unusual optical adjustment employed in the experiments with the sliding stage stereoscope. In order to unite *m* and *n* we must make the same separate adjustments of refraction and axial convergence as when using the upper stage and forming the resultant beyond the drawings—and to combine *s* and *t* we must proceed as if using the lower stage and viewing the resultant nearer than the drawings. But it is obvious that

the best practicable adjustment of the lenses and diagrams can only secure the direct optical coincidence of the virtual foci first referred to for certain points or lines of the drawings, and must leave all the rest to be combined by the method of constrained or unusual adjustments of which I have been speaking.

The same thing occurs in using the reflecting stereoscope of Wheatstone (fig. 7), if after adjusting the diagrams so as to cause the right and left images to appear side by side instead of being coincident, we then by constrained vision bring them together so as to form the resultant in a position nearer or more remote than the plane in which they are situated. Thus if m and n denote the virtual foci or images of P and Q, formed by reflexion from the inclined mirror C and D, these by ordinary vision will appear to the eyes at A and B as separate objects actually placed at m and n. But we may readily unite them beyond R by converging the optic axes to the proper point while we maintain the refractive power in its previous adjustment to the distance of m and n from the eyes. Assuming the line VR to bisect the angle of the mirrors and HP KQ to be parallel to VR and equidistant from it, and supposing PQ, to be at right angles to it, it is evident that P and Q, considered as points in the drawings, will have their virtual foci optically coincident at r, and will be seen as one in that position. If however we move them backwards on the lateral slides of the stereoscope to PQ, equidistant in the same direction from PQ, their images will be found at m and n, and so when moved forwards to P_2Q_2, the same distance in front of PQ, their images will take the positions n and m, the reverse of the former. When however we adjust them in the position P_2 and Q_1, the former as far in advance of P as the latter is behind Q then the two images will be seen optically coincident at n, and so if placed at P_1, Q_2 they will form a united image at m.

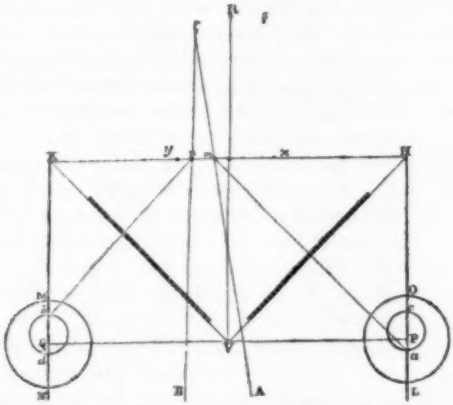
7.



From these relations we draw the general conclusion, that supposing the stereoscopic drawings to be duly adjusted in the slides, the points situated at P and Q and such other pairs of points as are equidistant from PQ in opposite directions will all be united pair by pair in different positions on the line HK; all pairs of points situated in front of PQ or behind it, or on opposite sides

of it but at unequal distances from it, will form separate virtual foci on HK. The former will of course be seen united by the natural process of vision as if the coincident foci were real objects; the latter will appear separate, one to each eye, and will require for their union the convergence of the axes to some point either nearer or more remote than the pair of images, at the same time that the refractive power of the eyes is maintained in its adjustment to the distance at which the images are placed.

8.



To apply these considerations to a particular case in which a perspective resultant is formed, let the drawings placed in the stereoscope (fig. 8) be those which by their union form a hollow truncated cone and let the centres of the two external circles be placed at PQ. In this adjustment the images of L and M will be optically united on HK at a point x on the left of the centre of the line and at a distance equal to the radius of the circle, and the images of N and O will be united at a point y at an equal distance from the middle of HK towards the right. All the other corresponding points of the two exterior circles will be optically united in the vertical plane of HK, so as to form an equal resultant circle in that plane, having its centre in VR. Turning now to the smaller circles it is obvious that the corresponding points a and b form separate images at m and n respectively which in virtue of the forced or unusual adjustment are united at r. In the same way we are presented with the resultant image of c and d at s in a symmetrical position on the opposite side of VR, and thus the resultants of all the corresponding points of the two smaller circles unite to form a circular image in the position rs parallel to and behind the resultant circle at xy. The diameter of this image is greater than that of the component circles ab or cd in the same proportion as its distance from the eyes at AB exceeds that of HK from AB.

It thus appears that in the positions above chosen for the drawings, the combination of the two smaller circles into one in the perspective resultant involves the process of constrained visual adjustment, while that of the larger circles is directly effected by their optical union in the vertical plane of HK. But if both drawings be moved towards HK until L and N are at small distances within P and Q, the eyes at AB will see the images in the vertical plane of HK on opposite sides of the central line and at a distance asunder equal to twice the joint distance of L and N from P and Q. These images will undoubtedly have exactly the same relations to the eyes as the drawings would have if placed on the upper stage of the sliding-stage stereoscope, to be united by converging the axes to points beyond them.

We see therefore, that with the most perfect adjustment of the drawings in a stereoscope, many parts of the resultant pespective figure are formed by a forced union of images that do not optically coincide, and that they become distinctly visible only through a separate adaptation of the refraction and the axial convergence of the eyes. Such indeed is the condition under which we see all parts of the perspective resultant not included in the vertical plane (that of HK fig. 8) in which the images of the drawings would be separately seen.

In describing the use of his beautiful invention, the reflecting stereoscope, Prof. Wheatstone appears in part to have overlooked the considerations above mentioned, for he says (Phil. Mag., 4 series, vol. iii), "the two pictures or rather their reflected images are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment," and again in the same paper, "there is only one position in which the binocular image will be immediately seen single of the proper magnitude and without fatigue to the eyes, because in this position only, the ordinary relations between the magnitude of the pictures on the retina the inclination of the optic axes and the adaptation of the eye to distinct vison at different distances are presumed!" This statement as we have just seen is true only of those parts of the resultant which lie in the vertical plane of HK, but in regard to the rest and by far the greatest part of the perspective figure, the "ordinary relations" mentioned by Prof. Wheatstone are not preserved.

I would remark in conclusion, that this necessity of departing from the usual adjustment of the eyes is no doubt one of the reasons that so large a number of persons having sound and equally balanced eyes, are unable on first looking into a stereoscope, to perceive the resultant figure clearly and steadily, and that even experienced observers are obliged in the case of certain figures to use considerable effort in order to effect the binocular combination.

ART. XI.—*Researches in Magnetisation;* by M. JEROME NICKLÈS.

IN two memoirs, published in the numbers of this Journal for January and March, 1853, treating of the elongation of magnetised bars and of the influence this elongation exerts on the attracting force, I have admitted that the attraction will increase with the distance that separates the two poles of the bar, resting this conclusion on the consideration that in the separation of the poles, the effects of neutralisation which they exert between themselves will be diminished. After giving some proofs to this end, I show that the influence excited at a given time may change its sign, and be reduced to zero, if the electromagnet have a horse-shoe shape* so that both poles act on the armature.

These facts have since been verified, and they authorise our inferring what may happen, when, the current or helix being unchanged, we vary the distance between the polar branches of a bifurcate electromagnet. For, on increasing this distance, the mass of the iron of which the electromagnet consists is increased, or, what is the same, the polar branches are elongated, which occasions a tendency to augmentation of force; the chances of neutralisation between the two poles are diminished, whence results another tendency to increase of force.

It is easily seen that the two tendencies are not of the same kind; one may be at zero, when the other exerts its whole effect: by separating to a greater distance the parallel branches, there is no change in the relative situation of the helices or of the branches, while the position of the poles with reference to the armature is altered: for it is evident that the armature intercepts more magnetic rays when the poles are remote than when they are very near.

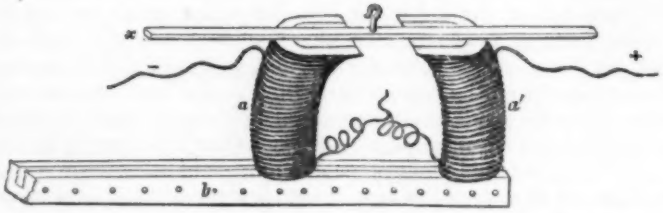
In the course of my researches on circular electromagnets, I have often verified this conclusion, and as the laws of these electromagnets are the same as those which govern bifurcate electromagnets, it may be seen that the separation of the poles will also have some effect on the power of these magnets. All phyicists are not of this opinion, and M. Dub besides others has just pronounced formally the contrary opinion, as a result of experiments whose precision I do not intend to question.

The facts which I have made known, while they do not contradict the results of his observations, weaken the force of his conclusions. To test it most satisfactorily, I operated, as he, with bifurcate electromagnets. The apparatus employed was a

* And not only in electromagnets which are wound with wire through all their length, as M. Dub observes in Poggendorff's Annalen, vol. xc. The same error is contained in the Jahresbericht of Liebig, &c., 1853, p. 248.

horse-shoe magnet (fig. 1), one leg of which is moveable and may be removed at pleasure. The joining piece, by which these two bars are made a horse-shoe magnet, is a rectangular iron bar, of convenient length, having a groove in the direction of the

1.



axis. The fixed leg a' is riveted to one of the extremities of this groove; the mobile leg has a shoulder which fits it to move in this groove; holes are made at intervals in the joining piece, so as to fix by means of a screw the moveable polar branch. The two branches terminate in a common straight line at their lower extremity; the upper extremity is recurved: the radius of the curve is greater than the radius of the helix, so that the two poles may be brought together till they touch by one of their sides if the branches are approached sufficiently near one another.

Perfect contact is not necessary in the demonstration which I would give; besides, as the legs may be indefinitely separated, it is easy to place them in extreme conditions, and so decide the question at a single trial. I have used a constant current, a needle being employed to test it.

Distance between the poles.	Current *a*.	Current *b*.
8 leaves of paper ($\frac{1}{2}$ mm.)	14–15 kil.	52 kil.
120 mm.	18 "	65 "

It is seen that the difference is very apparent. Now it was still to be ascertained whether these numbers express the limits, or whether the results vary with the separation of the poles. It was easy to observe, that with the intensities in play, the attraction no longer sensibly increased on going beyond a distance of 12 centimeters; that the distance favorable for the increase, augmented with the magnetic power developed and diminished when the current itself diminished. This may be seen in the following table which contains some of the mean results, observed at different intensities. I add that the battery used was made of plates of large size; that the branches aa' of the electromagnet were 15 mm. in diameter, and 9 centimeters long; that the helix contained each 47 meters of wire 1 mm. in diameter, and finally that the armature x, was a cylinder of iron 15 mm. thick and 30 centimeters long.

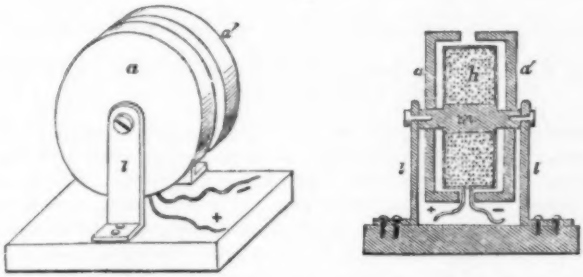
Distance between the poles.	Current *a*.	Current *b*.	Current *c*.	Current *d*.
Thickness of a leaf of paper,	5 kil.	10 kil.	17 kil.	45 kil.
0·0005 meter - -	8	14–15	22	52
0·025 - - -	10	16	23	55
0·045 - - -	10	18	25–26	58–59
0·120 - - -	9	18	27	65
0·220 - -	7	18	27	66
0·280 - - -	5	15	27	66

These results establish another analogy between the bifurcate electromagnets and the rectilinear. It is seen that the numbers which express the magnetising power increase at first regularly as in the last;* that they then decrease after passing a stationary point, variable with the intensity of the current or with the magnetism developed, and whose range augments with these intensities.

The residual magnetisms of the electromagnets employed exhibits the same relation after the interruption of this current; the armature falls spontaneously when the poles are at a small distance from one another; it remains suspended when this distance is increased; finally it falls again when the separation has reached a certain point.

Analogous facts have been observed with a circular electromagnet† constructed thus: it consists (fig. 2) of two disks of iron

2.



aa' 9 centimeters in diameter, and 2 in thickness, excavated to a depth of 8 millimeters; these two disks are put on an axis *m*, 35 mm. in diameter; the helix *h* is wound on the middle of this axis, and the helix made narrow so as to be enclosed by the disks; the disks are moveable, and can be either put in contact or removed at will to 15 mm.; they are fixed on a brass support *l*. With this apparatus, the increase of force produced by the separation of the polar circles is so sensible that it is recognisable by a simple touch with an armature in spite of the different causes which tend to vary the effect. The following are some of the results.

* This Jour., xv, 318. † This Jour., xvi, 180.

Dist. between the circles.	Current *a*.	Current *b*.
Contact,	1 kil.	1½ kil.
Thickness of a leaf of paper,	3	5
1 mm.	5	10
2 "	9	12–13
10 "	9	15
14 "	7	15

These facts added to the preceding explain the results obtained by M. Dub, as well as the conclusions which he draws. This physicist, not having sufficiently extended the limit of the separation of the legs of his electromagnet, his limits varying between 2½ and 5¼ inches, obtained nearly invariable numbers, like those which I myself obtained under the same circumstances (Table 2).

Practical Conclusions.—To the conditions required in the construction of straight, bifurcate, or circular, electromagnets, should be added,—giving the poles a remoteness corresponding with the magnetic intensity to be developed; the mean distance for bifurcate electromagnets of the ordinary size may vary between 6 and 12 centimeters, which corresponds to the distance commonly used. It is necessary absolutely to reject an arrangement like that given in the work entitled, *American Electromagnetic Telegraph*, an arrangement which requires that the poles should be brought almost in contact.

ART. XII.—*Correspondence of M. Jerome Nicklès, dated Paris, March* 1, 1855.—(*Continued* from vol. xix, p. 416.)

Annual Session of the Academy of Sciences.—Distribution of Prizes.—This session was opened, as is usual, with discourses and historical eulogiums. The distribution of prizes next followed. The Cuvierian Prize, which is given only for works of the first merit, was presented to M. J. Müller, for his Researches into the Structure and Development of Echinoderms, one of those works "which have contributed most to the philosophy of the science, to organogeny, zoology and general physiology, since the death of Cuvier." This is the second time the Cuvierian Prize has been given, it having been awarded for the first time in 1852 to Agassiz, for his work on Fossil Fishes. A prize of 2000 francs was given by the Section of Medicine to M. Berthelot for his chemical researches on the fatty bodies. Foreign men of science as usual, have received a share in this distribution; among them Wilhelm Boeck and Danielson for their Treatise on the "Elephantiasis des Grecs," and M. Schiff of Frankfort, for his memoir "on the influence of the nerves on the nutrition of bones." Medals were decreed to all the astronomers who have during the year 1854 discovered planets—to MM. Luther, Marth, Hind, Ferguson, Goldschmidt, and Chacornac.

Three awards were given for improvements in the processes used in the Arts that are injurious to health—one for the substitution of potato starch

for wood charcoal in preparing moulds of clay for receiving copper, bronze and melted cast iron, proposed by a poor armorer, M. Rouy ; a plan now generally adopted in the founderies in France, because it is not so hurtful to the workmen, although starch is dearer than charcoal powder. Another award was made to M. Mabru for a process for preserving milk in its natural state, which is simply this—tin canisters having a small tubular opening are filled full and then kept for some time in a water-bath to drive off all the air, and finally hermetically sealed.

Spongy Metals used in therapeutics.—We have already spoken of the spongy metals which M. Chenot has obtained by reducing the oxydized ores by means of a gaseous mixture of hydrogen and of oxyd of carbon produced through the decomposition of steam by incandescent charcoal. M. Chenot now proposes the use of these sponges for cauterising. In a trial which he has made, the blood was immediately coagulated, water absorbed, then decomposed, the oxygen becoming united to the metal and the hydrogen set free : these two effects give place to an energetic local action due to the disengagement of heat produced by the reaction. The application of the sponge of iron modified promptly and beneficially the suppurating parts, and brought about in a short time large and profound ecchymoses, etc.

Telegraphic messages simultaneous in two directions.—Much has been recently said on the communication of telegraphic messages in opposite directions on the same wire at the same time, which was first done the last year by M. Ginth, director of the Austrian telegraph lines. The signals were obtained by an electro-chemical way, the electro-magnetic action not answering in this case.

These facts call to mind what I remarked during the last year on a memoir on the heating of a conducting wire by the passage of two opposite currents according to an experiment made by Provostaye and Desains. I do not consider it a simultaneous passage of electricity in opposite directions in one case any more than in the other. If the electro-chemical telegraph is effective, when the electro-magnetic is in action, it is because the chemical action demands for its exercise less time than that required by the deviation of the needles or the attraction of the electromagnets. It is hence conceivable, that the exceedingly small difference of time which exists between two pulsations at the two extreme stations, these being never absolutely at the same moment, will be sufficient for a chemical impression and not for a mechanical impulse. M. Zantedeschi, of Italy, who has seen the Austrian experiments, observes that not only contrary currents may be propagated ; but also that light and physiological effects may be obtained by opposing two equal induced currents. Experiments of great exactness made by M. Gaugain with Ruhmkorff's apparatus have demonstrated that this cannot be, and that two equal induced currents opposite and synchronous wholly neutralize one another.

Calcium, Barium, Aluminium.—M. Bunsen has sent to the Academy a considerable quantity of metallic calcium which he has prepared by means of the pile. Its color is gray and lustre brilliant ; when heated, it fuses and burns with a bright light. He states that one of his students has prepared barium and strontium in the same way, and he is now studying their properties.

The production of aluminium is encouraged by the French government, who wish to procure a large quantity of this metal in order to use it for cuirasses and other purposes, for which it is especially fitted by its tenacity and lightness. The chemical establishment at Javelle near Paris, has engaged to furnish a certain number of kilograms of aluminium per week.

Pisciculture—The Piscicole Institution at Huningue (Haut-Rhin) distributed last year (1854) several thousands of fertile eggs of salmon, trout, &c. A large part of them were sent to the establishments founded on the model of that of Huningue, in France, England, Germany, Switzerland, etc. MM. Edmond and Thomas Ashworth of Ireland received eggs from Huningue for the establishment at Long Corrib, Ireland; and in 1854, 250,000 salmon were contained in their enclosures. M. Ramsbottom, under their direction, has established 350,000 on the borders of the river Tay, and M. Ayrton, as many more on the Dee.

The French government continues to sustain the establishment at Huningue. They make it also a school, where those who wish to learn the methods of manipulation may enter as pupils.

Correspondence of M. J. Nicklès, dated May 2, 1855.

Necrology.—The scientific world has of late suffered greatly by the loss of eminent men—Mathematics in the person of M. Gauss, and Natural Science by the death of Duvernoy, De la Beche and Greenough.

M. Gauss died on the 23d of February, 1855, at the age of 78 years. He was Free Academician of the Academy of Sciences at Paris, succeeding to the place of Sir Jos. Banks. M. Gauss represented in Germany what is known in France as the school of Laplace, that is to say, the metaphysical department of pure mathematics.

M. Gauss never shrank from any difficulty in the sciences. His theorem relative to the method of least squares, for arriving at the most probable or most correct of several slightly differing results—that for the motions of comets and of planets with their perturbations—that of the flexure and elasticity of surfaces forcibly moved from their primitive condition—and his algebraic labors in general, have all given him a place of the first rank in mathematical science.

His theory of the absolute value of the magnetic force of the terrestrial globe and the applications which have been made of it have enabled us to grasp clearly one of the elements of our spheroid. His theoretical discussion upon the division of the circle and on regular polygons, as well as his arithmetical studies upon the theory of numbers are regarded among the most abstract conceptions with which the human mind can be exercised.

Conceiving himself unknown or unappreciated, Gauss had withdrawn from the world for many years past: nevertheless his contemporaries were filled with admiration of his genius, although few among them were capable of reading his works understandingly and consequently of fully appreciating them.

Possibly it was for the purpose of making himself accessible to a greater number that Gauss took up the investigation of the Electric

Telegraph and from 1833 to 1836 he made, in connection with his pupil, Wilhelm Weber, the earliest useful experiments in Electro-telegraphing, in which he employed a battery, a magnetic needle, and a wire connecting the University of Göttingen with the Observatory.

L. G. Duvernoy deceased on the 1st of March, 1855, was born at Montbéliard (department of Doubs) the 6th of August, 1777. Till the age of fifteen he remained with his parents and received his early education under the eye of his father. He then went to Stuttgard, where he remained only a short time, because the province of Montbéliard having been added to the French Republic in October, 1793, young Duvernoy was required to return to France under the penalty of being considered as having emigrated. He continued his studies at the Academy of Strasburg, then filled with the recollections of the preceding generation in which were Cuvier, Goethe, Börne, the linguists Brunck, Schweighäuser, etc., and where he had for his fellow pupil the chemist Braconnot. He there pursued the study of the medical and natural sciences and had already made great progress in them when in 1799 he was called into active military service, entering as military physician in the army of the Alps, the left wing of the army of Italy. An epidemic typhus which broke out in the army gave him an opportunity to distinguish himself by his courage and devotion. The distinguished Parmentier, struck by the high courage and spirit of the youthful Duvernoy, wished to withdraw him from the hazards of war and obtained for him his discharge the following year. The earliest published works of this indefatigable laborer date from this time, his latest memoir was finished only one month before his death. The interest which so many distinguished men then felt in him was justified by several memoirs published in the journals of the day, the most remarkable of which appeared in 1799, under the title of "*Reflexions sur les corps organisés et les sciences qui en sont l'objèt.*" On his return to Paris he finished his studies and achieved his medical diploma by his thesis "*Sur l'hystérie.*"

Here begins the real scientific life of Duvernoy, with the date of his connection with that illustrious man who lived to found the sciences of Comparative Anatomy and Palæontology. Cuvier, who was his relative, recalled him to Paris in 1802, to aid him in numerous scientific labors, and having recognised his powers and his exactness as an observer, associated him with himself in his great work "*Leçons d'Anatomie comparée.*" This was in 1803. Cuvier had just published with M. Dumeril the two first volumes of this fine work. These volumes comprise the description of the skeleton, the organs of motion in general, the nervous system, and the organs of sense. To fill out this growing science, Cuvier took M. Duvernoy as his associate. Cuvier himself wrote out the general views which precede the chapters of descriptions in this work, as well as several of the latter, for example, that on *the teeth.* The rest was the work of our young anatomist, then twenty-six years of age. Having at command the rich collection which Cuvier had destined for this gigantic work, he dissected constantly, prepared his descriptions, his master only reviewing the proof-sheets. Cuvier often mentioned the value of the aid given him; he thus says in volume iii. of the "*Le-*

çons," "j'avoue cet'ouvrage comme le mien, tout en reconnaissant qu'il appartient aussi à M. Duvernoy, non seulement par la rédaction mais aussi par beaucoup de faits curieux dont je lui dois la connaissance." The last three volumes were published in the space of two years. This great labor did not hinder M. Duvernoy from publishing during this period nine memoirs on different subjects of physiology, of pathology and comparative anatomy. Such labors demanded a reward which Cuvier was impatient to obtain for his kinsman. In this he succeeded in 1809, when M. Duvernoy was named Assistant Professor of Zoology to the Faculty of Sciences in Paris, a chair then established. Cuvier hastened to recall to himself the young *savant* who having married in 1803, had returned, after the publication of the Leçons, to Montbéliard where he practised medicine. Duvernoy returned, to receive from Cuvier the highest possible mark of his confidence. Forced to absent himself, the great naturalist confided to M. Duvernoy the direction of all his scientific affairs.

The future seemed to open to M. Duvernoy a career of increasing celebrity: nevertheless he abandoned all, and again retired to honorable and laborious obscurity. His wife and children had remained in his native place. A deep sadness seriously affected his health, and on the 30th of December, 1809, he abruptly left Paris, abandoning a future of renown to find again his life and happiness in the joys of his family. Thus closed for a season the first period of the scientific life of M. Duvernoy. Devoted to the fatigues of medical practice, he toiled more than twenty years over the mountains of his native country to save and console the sick. His name rests in honor among the inhabitants there, and thither his mortal remains were borne. Duvernoy appeared then lost to the scientific world. Descriptions of fossil bones came at long intervals to call to mind the former labors of the physician of Montbéliard; but until 1827 it was feared that he was forever estranged from the field of scientific labor. About this period, at the age of 50 years, he reappeared broken by sorrows and seeking from science consolations for his misfortunes. His wife and several of his children had died in his arms. Happily, two of his daughters remained who with their second mother received all his affections and consoled his old age to the end of life.

In 1827 he accepted the chair of Zoology in the Faculty of Sciences at Strasburg and thus entered upon the second part of his scientific career. This had little resemblance to the first. Driven, contrary to his expectation, into an arena where new competitors had arisen, he was destined to meet new obstacles at every step. This only made him confident and more ardent in labor. Returning, from his retreat, to a science of which he had twenty-two years before laid the foundations, he found it enriched by new facts which he rapidly appropriated; and he prescribed for himself the mission of completing his unfinished labors of 1802. Cuvier still lived: but without ceasing to be the admirer of the great naturalist, Duvernoy remained free and independent, and honored his master by avowing, without fear of displeasing him, scientific opinions differing from his own.

Never was there a scientific life of greater labor than that of Duvernoy during these last twenty-seven years. They were divided into two peri-

ods, the first of which he passed at Strasburg and the second, which terminated with his life, in Paris. Before 1827, Duvernoy had never instructed, and he began the labors of his Professorial Chair with a review of the progress of natural history in the first twenty-five years of this century, and thus resumed his labors which were never again interrupted. The course with which he opened his career at Strasburg served as the point of departure for his original researches. In 1828 he remodeled, according to his own ideas, the classification of mammals, while in a series of 27 memoirs continued down to 1838, he described a crowd of anatomical facts, and cleared up many obscure points in Physiology. To this period belong his researches "Sur les serpents venimeux;" the monograph "des Musaraignes"; his studies "Sur le foie," the discovery "Des cœurs accessoires de la Chimère arctique;" "De l'organisation du cœur des Crocodiliens," &c. In 1832, Cuvier having died of an attack of cholera, Duvernoy sought to succeed him in his chair at the *Muséum d'Histoire Naturelle* at Paris but failed through a small deficiency in the ballot. The succession of Cuvier devolved on M. De Blainville, and M. Duvernoy achieved this honor more than 17 years later, upon the decease of this naturalist. This first check commenced the series of disappointments by which Duvernoy was to expiate his absence of twenty years. But he very soon gave his attention to the completion of his great work. Cuvier had left incomplete a second edition of his Comparative Anatomy. Duvernoy in 1837 commenced the revision of the part which he had edited in 1805. He made it a new work; the descriptions were verified from nature, the observations of other naturalists were severely scrutinized, and his own discoveries came in to enrich the facts already known. This work in five volumes was finished in 1846. Nominated Dean of the Faculty of Sciences at Strasburg in 1833, he did not resign these honorable duties until 1837 when he was entrusted with the chair of Natural History in the College of France. He was replaced by Dr. Lereboullet in the chair of Strasburg, who still holds it with distinction. His duties at the College of France commenced in 1838 and continued without interruption until 1850. The several branches of zoology were there passed in review. Finally in his later years he undertook a complete revision of zoological classification and proposed a method entirely his own. He hoped to put in print this last fruit of all his scientific studies, but death intervened to prevent. In the period from 1838 to 1850 he produced sixty-five memoirs consecrated particularly to anatomy and physiology. The memoirs upon the teeth of the Musaraignes (genus, Sorex) alone occupied eight years of persevering labor. In them he broached a new theory of the development of teeth.

M. Duvernoy was in succession, Correspondent of the Institute, and Free Academician; he occupied the chair of Cuvier at the *College of France* and at the *Muséum d'Histoire Naturelle*. On the 4th of December, 1854, he was seized with an attack of suffocating bronchitis. Escaping this first blow he sought to resume his occupations, but the disease made progress and he well understood that life was over with him. He assembled around him his family and his dearest friends to bid them farewell and leave them his love. M. Duvernoy desired to be interred at Montbéliard, his native city. Dumeril, the zoologist, the

companion of his youth, accompanied his remains even to their destination notwithstanding his great age and the severity of the season.

Magnetic force of Oxygen.—In 1849 M. Edmond Becquerel, Professor at the "Conservatoire des Arts et Metiers" in studying the action of electro-magnets upon various bodies, established that oxygen is magnetic and that atmospheric air in virtue of the oxygen it contains, partakes of the same property. The mode of experiment to measure the force exercised by a magnet on a gas in comparison with the effect produced upon a body taken as unity, consisted in placing successively small bars of glass, wax, &c., in a vacuum and in different gases, in order to determine the magnetic power of the gas by the difference of effect produced under these two conditions. He thus established that the relation between the attraction of oxygen by a magnet, and the repulsion in an equal volume of water, is proportioned to the density of the gas; and that it is represented by 0·18 at the temperature of 12° C.

If we reflect that the earth is surrounded by a mass of air equivalent in weight to a stratum of mercury 76 centimeters in height, we can understand that a similar mass submitted to the incessant variations of pressure and temperature, ought to exercise an influence on some of the phenomena dependent on terrestrial magnetism. In calculating what is the real magnetic power of this fluid mass we find that it is equivalent to an immense shell of iron of a thickness of $\frac{1}{10}$th of a millimeter, covering the entire surface of the globe. M. Becquerel's results have been confirmed by M. Faraday* and by M. Matteucci by different methods. M. Plucker having reached other results by the use of a method of his own—a process by weight—Becquerel has revised his researches and confirmed anew the results previously announced.

Oxygen in the nascent state—Ozone.—For some time past, observations have been made in Europe on atmospheric ozone. Owing to the persevering efforts of MM. Wolf of Berne, D. Bœckel of Strasburg, and Dr. Simonin of Nancy, some general facts of the highest interest have been reached by the use of Schönbein's test (i. e. paper made sensitive by starch and iodid of potassium). According to these observations there exists an intimate relation between the quantity of ozone in the air and certain epidemic diseases such as cholera, grippe, intermittent fever, &c. They think they have established that the appearance of the grippe coincides with the presence in the air of an excess of ozone: that on the contrary the invasion of cholera is accompanied by an almost complete absence of ozone in the air; this is at least true for the places above named.†

It is well known that ozone is regarded as an isomeric or allotropic condition of oxygen. MM. Becquerel and Frémy have called it electrized oxygen and have prepared it, as we have before described, by submitting pure oxygen to the electrical current.

The following is a new mode of preparing it in abundance (or at least a *similar* body) capable of oxydizing silver, of decomposing iodid

* Faraday's results were published before 1849, in 1847, his first paper on the magnetism of oxygen having been read before the Royal Society in that year, and published in the Philosophical Magazine for Dec. 1847.—Eds.

† It will be remembered that in 1849 much was said in the medical journals of this land, on the relation between cholera and ozone, the absence of ozone being insisted on at that time in cholera districts in the United States.—Eds.

of potassium, of burning ammonia, of disengaging chlorine from hydrochloric acid, and of forming water with hydrogen. This simple process consists in treating peroxyd of barium (BaO_2) with monohydrated sulphuric acid at a temperature below 70° C. The oxygen disengaged in this process possesses the properties named above, and it has the characteristic odor which is known as the Lobster odor. M. Houzeau assistant to M. Boussingault, is the author of this process which he discovered during a series of researches on the preparation of oxygen from the peroxyd of barium by heat.

Attempts to insulate Fluorine.—M. Frémy has been for many years engaged in this research, employing as the means of decomposition the galvanic pile, and selecting for his experiments the fluorid of potassium obtained by the calcination of the fluohydrated fluorid of the same base ($FlH+FlK$) in a platinum crucible. The resulting fluorid is of difficult fusion and in order to submit it to the action of the pile, M. Frémy introduces it into a tubulated retort of platinum which is then submitted to the heat of a good forge fire. A platinum wire communicates the currents from the positive pole and enters the fused fluorid; the negative pole connects with the retort. The platinum reddens rapidly under this action, and is changed to fluorid of platinum which at that temperature is at once decomposed. From the neck of the retort a gas issues, which is strongly odorous; it decomposes water producing fluohydric acid and displaces the iodine of the iodids. M. Frémy regards this gas as fluorine, and he has already prepared it many times in his laboratory at the Polytechnic School. It is in other respects identical with what has been previously obtained by the decomposition of certain fluorids by oxygen at a high temperature.

Aluminium, Silicium, &c. &c.—The experiments alluded to in my last communication are continued with zeal in the hope of producing aluminium in an economical manner. They have commenced by cheapening the production of sodium the present selling price of which in the large way has been reduced to 10 francs the kilogramme, (about 90 c. per lb.). We have already described in a preceding number the improvements which M. Deville has introduced into this manufacture.

Spongy Metals.—We have several times spoken of spongy metals and especially of the spongy iron of M. Chenot who first recognised the importance of employing aluminium as an alloy. He has for many years prepared specimens of very hard steel composed entirely of iron and aluminium—and which have received high commendation from most manufacturers. M. Chenot has often remarked, that in compressing spongy iron, the production of a harsh sound accompanied the rupture of the moulds employed. Very recently in compressing spongy silicium this fact was reproduced in a very marked manner. Three grammes of silicium in the spongy state having been submitted to a pressure equal to 300 atmospheres, it exploded with a fearful noise—the fragments of steel from the broken matrix entered many millimetres into a plate of cast iron, and the body of the hydraulic press which was 20 centimeters in thickness was broken, and this, although the safety valve was open, thus showing the sudden violence of the shock. The action was entirely from above downward since no portion of the upper part of the compressed metal in this case suffered.

This phenomenon has analogies with the fact published by Mr. Gore in the Philosophical Magazine, namely, the explosive property of antimony when loosely arranged to favor its combination with chlorine under the influence of the pile. These facts open to us a new property of metallic substances, due to a particular mechanical state or condition. Possibly it is to this cause that we must refer the explosion so often noticed in the manufacture of potassium and sodium, the cause of which has never been explained, although chemists have frequently sought for it. Recalling the power of spongy platinum in the condensation of gases, we are led to enquire, if the phenomenon noticed by M. Chenot may not be of the same order. The spongy silicium previous to compression had been exposed to air, condensing in its pores a large volume of gas. The strong pressure, to which it was afterwards submitted, suddenly set at liberty the gas by diminishing the capacity of the pores, producing an explosive effect analogous to the sudden evolution of steam from water in the spheroidal state.

Paris Universal Exhibition.—The postponement of the opening of the French Exhibition to May 15, and then to June 1, has long been a recorded fact. It is not our place to describe what is going on in this great cosmopolitan palace, where nations are displaying their chefs d'œuvre. English astronomy will send forward a full size model of the great Greenwich circle executed under the direction of M. Airy at the expense of the British government. The telescopes of Lord Rosse, of M. Lassell, and of M. Nasmyth, will be represented also by models. Mr. Piazzi Smith will send certain astronomical instruments. The new observatory will transmit its complete collection of magnetic and meteorological instruments which will be mounted and placed in action at Paris under the direction of Mr. Welch. The Bureau of Ordnance will exhibit the great theodolite which it has employed in triangulation, and examples of charts on different scales. The department of geological charts has also prepared a collection which will give a complete idea of its labors. The men of science in England at first felt some repugnance to exposing their physical instruments, like manufacturers, but this feeling has given way, and the number of exhibitors in this class will be a hundred or more, among whom appear, MM. Herschel, Brewster, Sabine, James, Willis, Lord Rosse, Wrottesly, Snow Harris, Wheatstone, Lassell, Grove, Warren de la Rue, Arnott, Gassiot, Brodie, and Frankland. Many of the great institutions figure also in the list, viz., University College, Guy's Hospital, &c. Every facility is offered for the exhibition of apparatus and machines in electricity which have not yet received a definite application. While the electric telegraph and telegraphic apparatus will be very severely scrutinized, being regarded as a matured subject, they will be extremely indulgent to machines for electrical motion, as this application stands in need of encouragement. Among machines of this class, will appear the motor apparatus described in this Journal, 1853,* consisting of 4 equal electromagnets acting in succession upon 6 cylinderical armatures placed around the magnets which receive the current alternately by means of four circuit breakers: this apparatus is in active use in a machine shop in Paris, No. 10 Rue

* July, 1853, p. 110.

Bichat. A celebrated juggler, Robert Houdin, will exhibit an electrical pendulum in which he has surmounted two important difficulties, to wit, the disturbing influence of variations of current upon the motion of the pendulum, and the destructive influence of contact breakers. The construction of this pendulum has given to horology a new mechanism which will have important applications.

Photography.—Employment of the Cyanid of Iodine.—M. Stéphane Geoffray, Advocate at Roanne, and a great amateur in the art of photography, has employed with success the cyanid of iodine as the sensitive agent in direct positives. This compound he obtains by the action of iodine upon cyanid of mercury. It is very soluble in a solution prepared with wax and benzine, and gives to this coating a rapidity of action nearly equal to that obtained by the use of ceroleine. Applied to collodion in the proportion of other iodids, the cyanid uniformly affords direct positive proofs of great beauty and which do not subsequently change. After fixation in the old bath of hyposulphite, the lights become very beautiful and are not inferior to those given by the use of the sesquichlorid of mercury.

M. Geoffray also employs the sesquichlorid of iron for all the purposes for which the sesquichlorid of mercury has been hitherto used, to bring out negative proofs on collodion and upon albumen in the manner employed for direct positives. He also employs this agent in place of the iodid to prepare sensitive papers for making positives in the shade and in a few seconds; according to the process of M. Blanquart-Ewrard. With this chlorid he also prepares a dry collodion of excellent quality, as follows:

To 100 grammes of ordinary non-sensitive collodion he adds 50 centigrammes of dry and finely pulverized perchlorid of iron, having no acid reaction; he boils it for a quarter of an hour, and adds four drops of tincture of iodine and filters the mixture. The glass being perfectly cleaned, he pours on the collodion, waits a moment to allow the coating to acquire a certain solidity—then plunges it in a bath of nitrate of silver, again in distilled water, and finally is dried, protected from the dust. The image is revealed as usual by pyrogallic acid.

This collodion is more sensitive than that prepared by using the protochlorids, but is much less so than the moist collodions.

Artificial Alcohol.—In my last communication I neglected to speak of the production of alcohol by means of water and illuminating gas. M. Berthelot has reached this important result through a species of contact between $C_4 H_4$ dissolved in fuming sulphuric acid and water contained in the acid. This important discovery has been the subject of a Report to the Academy of Sciences by M. Thénard. In this Report (a very flattering one to the young chemist) the venerable Dean of French chemists points out several other attempts of M. Berthelot and among them that of converting grape sugar into cane sugar. In spite of certain difficulties, we may still believe this a possible result. Nevertheless M. Biot doubts the possibility of this change, because it requires that the intimate molecular structure of the substance should be changed, a change to which we have no analogy in the transformations hitherto made known. This difficulty however does not appear to MM. Thénard and Dumas as insurmountable; since in treating cane sugar

with an acid, its molecular constitution is changed so that its rotatory power over the polarized ray is reversed from right to left, why then should it be impossible to convert left-handed rotaton to right-handed?

Late changes in the scientific corps at Paris.—A considerable movement has taken place among the scientific men living in Paris—or who pass the larger part of their time in that city.

FAYE the astronomer and member of the Institute has been appointed Provost (recteur) to the Faculty of Sciences at Nancy. M. MALAGUTI has been made Dean of the Faculty of Sciences at Rennes. M. PASTEUR has been made Dean of the Faculty of Sciences at Lille. M. GODRON, the author of the "*Flore de France*," has been nominated Dean of the Faculty of Sciences at Nancy. M. MORREN has been made Dean of the Faculty of Sciences at Marseilles. M. DESAINS has been appointed Professor of Physics to the Faculty of Sciences at Paris. M. FOUCAULT has been nominated as Physicist to the Paris Observatory. M. LECOQ is made Professor of Botany at Clermont. M. LALLEMAND to be Professor of Physics at Rennes. M. FAVRE to be Professor of Chemistry at Marseilles. M. GERHARDT to be Professor of Chemistry at Strasburg. I may also add that the writer has been appointed Professor of Chemistry to the faculty of sciences at Nancy.

Bibliography.—Etudes physiologiques sur les animalcules des infusions végétales, comparés aux organes élémentaires des végétaux par M. PAUL LAURENT, professor à l'Ecole forestière de Nancy; grand in 4°, orné de 22 planches dessinées et lithographiées par l'auteur. New York, chez H. Bailliere.—The first volume of this work is now ready. This book contains the fruits of twenty years assiduous labor, and throws entirely new light upon the animalcules contained in vegetable infusions (infusoria); their origin and development and their habits are here exhibited in the most interesting manner. Through new methods (which he describes with care in his work), M. P. Laurent has been able to observe better than his predecessors, to group facts and phenomena which had previously escaped notice, and to reunite in one species individuals which have before been regarded as wholly distinct. This first volume has made a great sensation among micrographers, unhappily rare in France. The second and last volume will not be less curious.

Mecanique analytique, de Lagrange. 3d edition revised, corrected and annotated, by M. Bertrand; t. ii, Paris, Mallet-Bachelier.—We have already announced the publication of the first volume of this work. The second volume treats of Dynamics. It is concluded by several fragments either by Lagrange himself or his contemporaries.

Œuvres Complétes de Fr. Arago, t. iv, containing the historical Eulogies of Malus, Gay-Lussac: the biographies of Copernicus, Tycho Brahe, Galileo, Newton, Kepler, Huyghens, Cassini, Lacaille, Roemer and a great number of other astronomers and physicists.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *On the Expansion of certain substances by Cold;* by W. J. Macquorn Rankine, Civil Engineer, F.R.SS., Lond. and Edinb. &c. (Phil. Mag., [4], viii, 357.)—During the discussion which followed the reading of the Rev. Prof. Powell's Report on Radiant Heat, at the meeting of the British Association in 1854, it appears, from the report in the *Athenæum*, that a question was proposed by Dr. Stevelly as to the means of reconciling the dynamical theory of heat with the fact of the expansion of water, antimony, cast iron, and bismuth, by cold, at and near their freezing points; and that some observations were made in reply by Professor Powell and Prof. William Thomson.

The question is one of much importance, and calls for the attention of every one who has been instrumental in maintaining the dynamical theory of heat. Having been prevented by illness from attending the meeting of the Association, I beg leave to offer the following remarks for publication in the Philosophical Magazine.

The theory of thermo-dynamics, strictly speaking, is a system of propositions, all of which are deducible from the following two laws:

I. Heat is convertible with mechanical power.

II. The mechanical equivalent of the quantity of heat which disappears or appears during any indefinitely small change (dV) of the volume of a given mass of a given substance, at a constant absolute temperature (τ), is the product of the following three quantities: the variation of volume, the absolute temperature, and the rate of variation of the expansive pressure (P) with temperature at constant volume; that is to say,

$$\tau \frac{d\mathrm{P}}{d\tau} d\mathrm{V}.$$

When this product is positive, it represents heat which disappears; when negative, heat which appears.

Neither of those laws indicates any particular relation between the pressure, volume, and temperature of a given mass of a given substance as more probable than any other. The nature of such relations must be determined for each substance by experiment, before the two general laws above stated can be applied to it.

Hence it appears that it is not the province of the theory of thermo-dynamics to explain the fact of the expansion of certain substances with cold, but simply to deduce the consequences of that fact, so far as they relate to the quantities of heat which are made to appear and disappear by effecting given changes in the volume of those substances.*

It is true that if we frame a *hypothesis* as to the molecular structure of matter (such as the hypothesis of molecular vortices), so as to deduce the two laws of thermo-dynamics from those of ordinary mechanics, that hypothesis must lead to many consequences besides those two laws; and it is necessary that those consequences should not be inconsistent with any of the phenomena of the relations between the tem-

* One of those consequences was anticipated by Mr. James Thomson, and proved experimentally by Professor William Thomson, at a time when the theory of thermo-dynamics was in a very imperfect state, viz., that the freezing-point of water is lowered by pressure.

perature, volume, and elasticity of bodies. No such inconsistency has hitherto been proved; but even were such inconsistencies to be proved, no objection to a molecular hypothesis can affect the certainty of the two laws of thermo-dynamics, which, though at first anticipated as the results of a hypothesis, have now been independently established by experiment.

With respect to the molecular mechanism that may be supposed to give rise to the expansion of certain liquids in cooling, near their freezing-points, it appears to me, that so far as our present knowledge enables us to judge, the most probable view is one concurred in by Professor Thomson, viz., that in a liquid near its freezing-point, there is an incipient tendency of the particles to assume a definite arrangement with respect to certain fixed axes; and that in substances which expand in freezing, that arrangement is such as to make the particles occupy more space than they do in the perfectly liquid state.

2. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; preceded by an Account of some Experiments on Molecular Influences;* by JOHN TYNDALL, Ph.D., F.R.S., (L. E. and D. Phil. Mag., [4], ix, 387.)—The paper commences with an introduction, in which the present aspect of the portions of science to which it refers is briefly sketched. A section is devoted to the examination of the magnetic properties of wood, which substance, the author finds, except where extraneous impurities are present, to be always diamagnetic, and to set in the magnetic field with its fibre equatorial. The influence of the shape of pointed and flat poles is studied, and those curious phenomena of rotation, first observed by M. Plücker, and attributed by him to the action of two conflicting forces, are referred to molecular structure as a cause. Between flat poles it is proved that the line joining the centres of the two poles is the line of *minimum* force; that is to say, the force increases more quickly from the central point of the magnetic field in an equatorial direction than in an axial one. Reflecting on the great diversity of opinion at present existing with regard to the real nature of the diamagnetic force, the author deemed it necessary to commence at the foundation of the inquiry. A fundamental question in the present case is the following:—Are diamagnetic bodies repelled by a magnet in virtue of any constant property possessed by the mass? Is the force in question a mere repulsion of ordinary matter, or is the repulsion exercised in virtue of a state of magnetisation into which the body is first thrown? This question is answered in a manner which admits of no doubt.

It is proved that the repulsion of diamagnetic bodies increases in a quicker ratio than the strength of the magnet which produces the repulsion. Within wide limits, indeed, the repulsion, instead of being simply proportional to the strength of the magnet, is proportional to the square of the strength, which leads inevitably to the conclusion that the body thus repelled contributes to the effect produced; that its repulsion is due to an excited condition into which it is thrown by the influencing magnet, the intensity of this excitement varying within the limits already referred to, as the strength of the magnet which produces it. This conclusion is further arrived at by a close comparison of the repulsions of diamagnetic bodies with the attraction of paramagnetic ones: both are found subservient to one and the same law.

It is next proved that the diamagnetic excitement produced by one pole of a magnet is not the state which enables a pole of an opposite quality to repel the substance :—that each pole induces a condition peculiar to itself, or, in other words, that the excitement of diamagnetic bodies in the magnetic field is of a *dual* character.

These points being established, a searching comparison is instituted between the phenomena exhibited by paramagnetic and diamagnetic bodies in three distinct cases :—first, when operated on by the magnet alone ; secondly, when operated on by the current alone ; and, thirdly, when operated on by the magnet and current combined. A bar of iron was, in some of these cases, compared with a bar of bismuth, but it was soon found necessary, in order to avoid the proved errors of reasoning, to take strict account of the molecular structure of the bismuth. A bar of this substance, cut in a certain manner from the crystallized mass, exhibits between the poles of a magnet precisely the same visible deportment as a bar of iron, while it is well known that the normal deportment of bismuth is opposed to that of iron. The author, in his examination of the points before us, divided paramagnetic bars into two distinct classes, and classified diamagnetic bars in the same manner ; one class he called *normal*, and the other class *abnormal*. A normal paramagnetic bar is one which sets its length from pole to pole in the magnetic field, and a normal diamagnetic bar is one which sets its length at right angles to the line joining the poles. An abnormal magnetic bar, on the contrary, is one which sets its length equatorially ; while an abnormal diamagnetic bar is one which sets its length axially.

In all cases, whether operated on by the magnet alone, the current alone, or the magnet and current combined, the deportment of the normal paramagnetic bar is precisely antithetical to that of the normal diamagnetic one. In the magnetic field the former sets axially, the latter equatorially. Operated on by a voltaic current, the former sets its length at right angles to the current, the latter sets its length parallel to it. When magnet and current act together on the bars, it is found that the disposition of forces which produces a deflection from right to left of the paramagnetic bar produces a deflection from left to right of the diamagnetic bar. If the position of equilibrium of the former be from N. E. to S. W., the position of equilibrium of the latter is from N. W. to S. E. In short, the position of rest for the normal magnetic bar is always at right angles to the position of the diamagnetic bar. A precisely similar antithesis is observed when we compare the abnormal paramagnetic bar with the abnormal diamagnetic one. The former, in the magnetic field, sets equatorially, the latter axially. The former sets parallel to an electric current, the latter perpendicular to the same. If the deflection of the former be from right to left, the deflection of the latter, under like conditions, is from left to right. Finally, the position of equilibrium of the former is always at right angles to that of the latter.

But if the deportment of the normal paramagnetic bar be compared with that of the abnormal diamagnetic one, it will be found that they are in all cases identical ; and the same identity of deportment is exhibited when the abnormal paramagnetic bar is compared with the normal diamagnetic one. The necessity of paying attention to structure in exper-

iments of this nature, could not, it is imagined, be more strikingly exhibited.

It is proved by these experiments that the simple substitution of an attractive force for a repulsive one would completely convert the phenomena exhibited by paramagnetic bodies into those exhibited by diamagnetic ones; that if that which Gauss has called the ideal distribution of magnetism in an iron bar be reversed, we have a distribution which would produce the phenomena of a bismuth bar of the same dimensions. All the phenomena of diamagnetic bodies become equally intelligible with those of paramagnetic ones, when we assume that the former class possess a polarity the same in kind, but the opposite in direction, to that of the latter.

It is well known that a bar of iron surrounded by a helix in which a voltaic current circulates is converted into a magnet, and exhibits that *twoness* of action,—those phenomena of attraction and repulsion at its two ends—to which we give the name of polarity. The present paper contains an account of experiments made with the view of ascertaining whether similar phenomena were exhibited by a bar of pure bismuth. A cylinder of the latter substance, 6½ inches long and 0·4 of an inch in diameter, was suspended by a suitable device within a helix of covered copper wire, so that it could vibrate freely from side to side. The ends of two electro-magnetic cores were brought to bear upon the two ends of the bismuth bar, and it was so arranged that the two magnetic poles acting upon the bar might be of the same or of opposite qualities. The helix being first excited by a strong current, a current of considerably less power was sent round the electro-magnetic cores, and their action upon the bismuth bar was observed: by means of a gyrotrope the current in the helix was reversed, and the effect of the reversion noted; permitting the current through the helix to flow in its last direction, by means of a second gyrotrope, the current which excited the cores was reversed; and finally, while the last magnetic polarity remained unaltered, the direction of the current in the helix was once more changed, and the consequent deportment of the bismuth bar was noted.

In making these experiments and exercising some judgment in the choice of the relation between the strength of the magnets and the strength of the current in the helix, the most complete mastery was attained over the motions of the bar. With one disposition of the forces the ends of the bar of bismuth were promptly repelled, with another arrangement they were just as promptly *attracted*. If, when moving towards the cores, the direction of the current in the helix was reversed, the motion of the bar was at once arrested, and attraction was converted into repulsion. If, while receding from the bar, the direction of the current in the helix was changed, the recession was stopped and the ends of the bar were attracted. The same results were obtained when, instead of changing the direction of the current in the helix, the polarity of the electro-magnetic cores was reversed. When the latter were so excited that the poles of the same quality were presented to the ends of the bismuth bar, the repulsion of the one pole was balanced by the attraction of the other, and under the influence of these opposing forces the bar stood still.

Pursuing the argument further, a south pole and a north pole were caused to act simultaneously upon *each* end of the bismuth bar; supposing one end of the latter to be repelled by a south pole, then, on the assumption of diamagnetic polarity, the same end would be attracted by a north pole; and permitting both poles to act upon it simultaneously from *opposite sides*, we may anticipate that the force tending to turn the bar will be greater than if only a single pole were used. To test this conclusion, *four* electro-magnetic cores were made use of; the two poles to the right of the bismuth bar were of the same name while the two to the left of the bismuth bar were of an opposite quality; with this arrangement the mechanical action upon the bar was greatly augmented, and the foregoing anticipation completely verified.

The bar employed in these experiments is unusually large, but it does not mark the practical limit of success. All the results obtained with this bar were obtained with another solid cylinder of bismuth 14 inches long and 1 inch in diameter. The corresponding experiments were made with bars of iron, and it was always found that the arrangement of forces which caused the attraction of the ends of the paramagnetic bar caused the repulsion of the ends of the diamagnetic one; while the disposition which caused the repulsion of the ends of the paramagnetic bar produced, in the most manifest manner, the *attraction* of the ends of the diamagnetic one. As regards the abstract question of polarity, the only difference between iron and bismuth is the comparatively intense action of the former. In the case of a magnetic body, whose capacity for magnetisation does not exceed that of bismuth, and in which coercive force is equally absent, no proof can be adduced in favor of the polarity of the former body that cannot be matched by proofs of equal value in every respect of the polarity of the latter.

The objections that have been and possibly may be used against diamagnetic polarity, are next considered, and some observations are made on the constitution of the magnetic field. The relation of our present knowledge of diamagnetic action to the theory of Weber, and to Ampère's hypothesis of molecular currents is stated; and in conclusion, the author dwells briefly upon those points of diamagnetic action wherein the views of M. Matteucci differ from his own.

3. *Further Experiments and Remarks on the Measurement of Heights by the Boiling Point of Water;* by Professor J. D. Forbes, (Edinb. N. Phil. J., [2], i, 174.)—This paper is in continuation of one printed in vol. xv. of the Edinburgh Royal Society's Transactions. The object of it is to test the correctnes of the method of observation and of calculating the results, then proposed, and to compare both with those of more recent authors, particularly of M. Regnault of Paris, and of Dr. Joseph D. Hooker.

The author finds the results of his subsequent observations in 1846 in the Alps, up to heights considerably above 10,000 feet, to agree well with those previously published, made in 1842. They combine in shewing a sensibly uniform fall of the boiling point at the rate of 1° for 543 feet of ascent (in a standard atmosphere at 32° of temperature), which differs only 6 feet (in defect) from his previous determination. The average deviation of the individual results from the formula is only $\frac{1}{12}$ of a degree (without regard to sign).

Barometer. Inches.	Boiling Point. Fahr.	Difference from my formula.	Difference from Regnault's formula.
20·77	194°·28	+ 0°·22	+ 0°·32
20·79	194·33	− 0·08	+ 0·01
22·40	197·94	− 0·04	+ 0·12
22·67	198·51	− 0·08	+ 0·06
23·15	199·52	− 0·07	+ 0·06
23·35	199·94	+ 0·01	+ 0·15
23·89	201·04	− 0·11	+ 0·03
23·99	201·24	− 0·09	+ 0·08
24·02	201·31	+ 0·04	− 0·20
24·105	201·47	− 0·17	+ 0·03
25·14	203·51	+ 0·04	+ 0·19
28·49	209·54	− 0·07	− 0.06

The agreement with M. Regnault's table is also extremely close; and considering the ordinary limits of error of such observations, the writer considers it nearly indifferent for elevations under 13,000 feet which method of calculation be used.

The consistency of the results shows that the method of observation (which differs in some respects from that commonly used) and the graduation of the thermometers were satisfactory.

On carefully examining Dr. Joseph Hooker's detailed results, (obligingly communicated by him,) which that naturalist considered to be incompatible with Professor Forbes's formula, it is shewn that the inconsistencies of observations are so considerable, that it is difficult to give a decided preference to one formula rather than another, for the purpose of representing them; but that up to heights of at least 13,000 feet, a *linear* formula, or one which assumes the lowering of the boiling point to be exactly proportional to the height, seems to express the observations as well as any other; and the rate of diminution is almost the same as that deduced from Professor Forbes's observations, or a lowering of 1° for 538 feet of ascent.

The author has little doubt that M. Regnault's table, (which was not published when he last wrote,) does really represent the law according to which water boils more accurately than the simpler linear formula, though the difference is in most cases insensible. For all ordinary heights (or up to 12,000 feet) Regnault's table may be more accurately represented by the formula, $h=535\,T$;
where h is the height in English feet, T the lowering of the boiling point in Fahrenheit's degrees, reckoning from 212°. But he finds that Regnault's table may be represented in every case which can occur in practice, and with almost perfect accuracy, by the following formula, which is nearly as easy to use:—

$$h=517\,T+T^2.$$

II. Mineralogy and Geology.

1. *Herrerite identical with Smithsonite;* by Dr. F. A. Genth, (Proc. Acad. Nat. Sci., vii, 232.)—When Herrera described an apple-green mineral from Albarradon in Mexico, having a distinct cleavage parallel with the planes of a rhombohedron, as a combination of 55·58 p. c. of tellurium, 12·32 p. c. of sesquioxyd of nickel and 31·86 of

carbonic acid, all mineralogists who were conversant with the laws of chemical combination, considered it as nothing more than a mechanical mixture, though the description appears to have been that of a simple substance. Afterwards Del Rio pronounced it "carbonate of zinc, with some nickel and cobalt," and this being a very probable composition, which also answers very well to the physical properties, it was generally considered a Smithsonite, though, (no other examination having been made of it,) as doubtful. I have received, through the kindness of Dr. J. L. LeConte, a genuine and perfectly pure piece of the mineral, which I have carefully examined. The physical properties of it agree very well with those given by Herrera. With regard to its chemical composition, Herrera has the merit of having correctly discovered in it the presence of carbonic acid; Del Rio, that of having found in it the ozyd of zinc. With regard to the other constituents, both were mistaken. A qualitative analysis has shown that this mineral contains carbonic acid, oxyd of zinc, oxyd of copper, oxyd of manganese, magnesia and lime, but no traces of any other substance. The quantitative analysis was made with the usual methods.

B.B. in a tube it blackens and does not give any "sublimate, which condenses in transparent drops," (as Herrera stated,) but none at all. On charcoal in the reducing flame it blackens, and covers it with white incrustations, having a steel-blue margin, which are yellow as long as hot; and which, when moistened with cobalt solution and re-heated, become green. The mineral itself does not become green, but brownish black; and after having received a good reducing heat, it shows under the magnifier, and when powdered in an agate mortar, metallic copper. With borax, it gives distinctly the copper reactions. Dissolves in acids slowly, with effervescence. After weighing, I have carefully examined all the separated substances for their purity, but neither traces of tellurium, nor those of cobalt and nickel, could be detected.

0·6226 grs. of the mineral gave:

0·3783 grs.	of oxyd of zinc, or	60·76 p. c	Zn O;
0.0137 "	" oxyd of copper, or	2·20 "	Cu O;
0·0062 "	" protosesquioxyd of manganese, or	0·93 "	Mn O;
0·0025 "	" pyrophosphate of magnesia, or	0·14 "	Mg O;
0·0092 "	" carbonate of lime, or	0·83 "	Ca O;

Herrerite is therefore nothing but a cupreous Smithsonite, and its composition the following:

Carbonate of zinc,	= 93·74	per cent.
" " copper,	= 3·42	"
" " manganese,	= 1·50	"
" " magnesia,	= 0·29	"
" " lime,	= 1·48	"
	100·43	

2. *Analyses of the Meteoric Iron from Tuczon, Province of Sonora, Mexico;* by Dr. F. A. Genth, (Proc. Acad. Nat. Sci., 1855, vii, 317.) —The masses of Meteoric Iron of Tuczon, first brought to notice by Dr. J. L. Le Conte, (Am. Journ. Sci, 2d Ser. xiii, 289,) and afterwards more fully described by Prof. C. U. Shepard, (Am. Journ. Sci., 2d Ser., xviii,

369,) are also mentioned by Dr. J. L. Smith in his Memoir on Meteorites, published in the last number of the same Journal, March, 1855, in which he gives the analysis of a piece cut from one of the huge masses by Lieut. Jno. G. Parke, of the U. S. Topographical Engineers. Several months ago I had finished the following analyses of the same meteorite, (which will be found to agree very well with those of Dr. Smith,) but various circumstances have heretofore prevented my presenting to the Academy the results of my examinations, which were made with pieces taken from the specimen presented to the collection of the Academy of Natural Sciences, by Dr. Heermann.

To the descriptions of this meteorite given by Prof. C. U. Shepard and Dr. J. L. Smith, I have to add only a few words.

The pieces which I have examined were not passive, as Prof. Shepard remarks, but dissolved readily in nitric acid, and also immediately precipitated metallic copper from the solution of the sulphate. On dissolving it in chlorohydric acid, only a slight odor of carburetted hydrogen was perceptible, and no gas evolved, which precipitated an ammoniacal solution of chlorid of copper; a very small quantity of Schreibersite separated in form of a brownish powder.

On evaporation of the solution by nitric acid in a water-bath and subsequent moistening of the dry mass by chlorohydric acid, all the substances were taken up, excepting a small residue of siliceous matter. This dissolved partly on boiling with carbonate of soda, leaving a residue, which I took for a feldspathic mineral; the quantity obtained, however, was too small for further examination; the small quantities of alkalies, lime and alumina speak in favor of this view, and indicate that the residue insoluble in carbonate of soda is labradorite, a mineral which is partly decomposed by acids. The silica, soluble in carbonate of soda, results undoubtedly from the decomposition of olivine. I was particularly careful to obtain the whole quantity of cobalt and nickel, and have therefore not separated the sesquioxyd of iron by carbonate of baryta, which method is not quite correct; but have precipitated the boiling solution, containing sesquioxyd of iron, oxyd of cobalt and nickel, magnesia, etc., and a sufficient quantity of chlorid of ammonium by ammonia, and immediately afterwards acidulated by acetic acid, boiled and filtered. All the other determinations were made in the usual way.

The following results were obtained:

	I.	II.	III.
Copper,	0·008	not estimated.	not estimated.
Iron,	83·472	not estimated.	83·637
Cobalt,	0·420	0·366	9·851
Nickel,	9·441	8·689	
Chrome,	not estimated.		0·174
Alumina,	traces.	traces.	traces.
Magnesia,	2·593	2·030	2·147
Lime,	0·463	0·550	not estimated.
Soda,	not estimated.	not estimated.	0·174
Potash,	not estimated.	not estimated.	0·098
Phosphorus,	0·103	not estimated.	0·150
Silica,	2·889	not estimated.	4·169
? Labradorite,	1·046	not estimated.	

3. *On the Physical Geology of the Himalaya;* by CAPTAIN RICHARD STRACHEY, F.R.S., F.G.S., (Jour. Geol. Soc., x, 249.)—The author having in a previous communication to the Society,* described the geological structure of a part of the Himalaya Mountains, in the present paper he proposed to complete that sketch, at the same time taking a rather larger field of view, offering some explanation of the manner in which the chain in general had been raised to its present position—first, with reference to the mechanical action of the forces in operation; and secondly, as to the epochs at which the upheavement took place.

Attention was first directed to some of the more prominent points of the physical structure of the great mountain mass, of which the Himalaya forms a portion. Among these were especially noticed:—1st. The general form of the section of the mass, which shows that the Himalaya mountains are merely the southern slope of a great protuberance, the summit of which forms the table-land of Tibet, while the northern slope is a mountainous region, marked on our maps as the Koneulun, similar to the Himalaya, and terminating in the great plains of Central Asia. 2nd. The parallelism to one another, and to the outer edge of the mountain area, of the great ridges or lines of elevation; of the great valleys or lines of drainage, which are also lines of rupture; of the strike of the strata; of the lines of igneous action; and of the distribution of the various deposits, considered both in reference to their mineral character and geological age. 3d. The arrangements of the drainage, in accordance with which the crests of the northern and southern slopes of the great mass form two main lines of water-shed, proposed to be termed the "Turkish and Indian Water-sheds," to the north and south of which, respectively, the rivers flow off directly to the plains of Turkistan on one side, and to those of Northern India on the other; while the waters of the summit of the table-land are collected into two streams alone, the Brahmaputra and the Indus, which are discharged from the mountains at two distant points, at opposite ends of the chain. 4th. The constancy maintained for great distances along the length of the chain, both in the geological structure, and in the elevations to which the mountains rise; which last character is particularly shown by the almost perfect horizontality of some of the most elevated stratified deposits, by the extremely small slope of the main drainage-channels of the Tibetan table-land, and by the general similarity of the altitudes even along the most rugged ridges.

After allusion to the question of the reality of upheavements, as opposed to the view which attributes elevations on the earth's surface to subsidences of the lower parts, and having stated reasons for believing that in the present instance the elevation was real, a review was taken of the igneous rocks which might appear to have been connected with the upheavements of these mountains, and the conclusion was arrived at, that none of them were specially related to the last great movement that had taken place.

Hence it was inferred that the agent of elevation was probably a development of elastic vapors, as has been considered by Mr. Hopkins to be likely in a general point of view. The probability of this in the

* Quart. Journ. Geol. Soc., vol. vii, p. 292

present instance was shown to be upheld by the the extreme regularity of the upheavement, when regarded as a whole, which seems to indicate a fluid body as the medium through which the forces have been applied to the superincumbent masses; while this regularity also shows the comparatively great thickness of the part of the earth's crust that has been raised, as compared to the heights of individual ridges or peaks above the general level.

Assuming the general unity of the upheavement of the whole area as evident from the consideration of the form of the section of the mountains, the results of such an upheavement were then examined by help of the principles laid down by Mr. Hopkins.

The first of these results was stated to be a main system of longitudinal fissures, parallel to the major axis of the elevated area; with a secondary system perpendicular to the former. As the manner in which these fissures would open, and all the subsequent effects of the upheavement, must depend in great measure on the state of the interior of the earth, of which we can know nothing directly, we must be guided in our suppositions on this point by the phenomena actually observed in connexion with the ruptures at the surface; and, in the present case, we seem to be able to account for many of the peculiarities of structure already noted, by conceiving that the state of the upheaved mass, at the moment of rupture, was similar to, but just the reverse of, that of a beam supported and held down at the ends, and loaded at the centre.

On this hypothesis we should expect to find the chief longitudinal ruptures in the centre of the area, with others important, but in a less degree, along the margins, while there would be two portions intermediate between the axis and the edges, where the tendency to rupture would be a minimum. Further, as the tendency to transverse rupture is proportional to the amount of the elevation, it would have a minimum along the edges of the area; at the same time, as in any part of the mass, the tendency to transverse rupture would be least where the general coherency was least disturbed, we should expect to find fewest transverse fissures on those lines of minimum longitudinal fracture just noticed.

Comparing these theoretical views with the observed facts, we find:—

1st. The existence in a marked degree, of the longitudinal and transverse fissures, in every part of the mountains.

2nd. The more open character and greater importance of the longitudinal fissures in the centre of the area, as evinced by the direction of rivers on the Tibetan table-land.

3d. The existence of an important line of fissure along the outer margin of the Himalayan slope, as proved by the narrow fringe of the latest formations that everywhere skirts the foot of the mountains, which it is impossible to suppose could have been raised as they are, excepting in connexion with some larger mass.

4th. The occurence of two lines of least rupture running parallel to the margin of the area, and intermediate between it and the axis, indicated by the Indian and Turkish water-sheds; these features being dependent for their existence, not on any superior elevation that they attain over other parts of the chain, but only on their continuity, and on the few transverse fissures by which they are broken through.

5th. The few transverse fissures along the outer margin of the area, which affords a probable explanation of the accumulation of the drainage of considerable areas of the Himalayan slope, into the few great rivers that pass through the outer ranges.

After entering into some details as to the more important lines of fissure, it was remarked that the presence of more fissures than may seem to be necessary, or of others not in accordance with these views, may be readily expected to have been caused in movements anterior to the last great elevation. At the same time, the general parallelism of all the anterior movements to the last is indicated by the constant parallelism of the strike of the strata, the lines of rupture, the lines of elevation, and those of the directions of the stratified deposits; although a certain obliquity at the same time does exist, which, however, must be more closely examined before more can be said of it.

The author then passed to the chronological part of the subject. The age of the granite of the great line of snowy peaks was first considered, and shown to be probably anterior to the Silurian period; but doubts still remain on this subject, though the occurrence of rolled pebbles of this rock in the tertiary deposits of the Tibetan table-land, show that the eruption was certainly antecedent to the middle of the tertiary epoch.

The existence of a coast-line along the Indian water-shed, as indicated by a conglomerate-bed at the bottom of the Silurian rocks, was next noticed, with the presumption of the existence of a northern sea, having its boundary along this line during the Silurian epoch, as well as during the Jurassic, and the earlier portion of the Tertiary period. On the Southern edge of the mountains there is nothing to prove the boundary of the land and sea, until the Tertiary period, when the coast-line is found along the general line of the existing outer hills. Still, the existence of fossiliferous beds in the salt-range in the north of the Punjab, apparently synchronous with those of the Indian water-shed, seems to render it probable that there was a Southern sea, contemporaneous with the Northern, extending over the existing planes of North India, from the remotest times, and leaving an area of dry land between them, on what is now the Himalayan slope. The probability of this area of dry land is further shown by the almost total absence of fossils in these regions; nor does the existence of fossils at one single point in Kashmir, where only they have been found, indicate more than an irregularity of the outline of the land, such as might have been anticipated.

Turning next to the question of the dip, which, as a general rule is everywhere towards the axis of the elevation, it was stated that there appeared to be nothing to connect this inclination with the last elevation, and that it is more probably due to some of the earlier movements of the surface. It was shown how an inclination, having been once imparted to any dislocated mass of the earth's crust, any subsequent motion would most likely maintain it, or even add to its amount; for the tilting of a mass displaces the position of the centre of gravity, so that it is no longer immediately over the centre of the base of the mass, at which the resultant of all elevating forces will act; and hence, in any subsequent elevation the weight of the mass will tend to make it re-

volve, by the relative descent of the centre of gravity, and ascent of the point of application of the elevating forces, which will evidently tend to increase the dip.

The dip of the outer hills, however, which is almost everywhere towards the interior of the chain, just as is the case with the rest of the mountains, cannot be attributed to these older movements, for these ranges are among the most recent of the whole; nor is there, at first sight, any very evident connexion between the dip that they might be supposed to assume, and that of the older ranges beyond them. The phenomenon may, however, perhaps be explained, by supposing that the fissure which we have reason to believe to exist along the foot of the mountains was originally covered up by the deposits which now form the outer hills, when the ocean extended over the plains of Northern India. An upheavement of the mountains alone, the general sea-bottom remaining unmoved, would naturally terminate at the fissure, and a narrow fringe of the younger beds would be raised on the flank of the older mass, to which a dip the same as that of the older mass might be imparted, by that tendency to revolve already explained. The repetition of this process would make a succession of ranges, such as are seen in the outer hills, apparently dipping under one another and the older beds, of the fragments of which they are evidently made up, in an inverse order.

The paper concluded by a recapitulation of the progress of the Himalayan chain, as far as it could be traced, from the earliest geological period to the present time. In this was pointed out the probable existence of land with mountains already of considerable altitude, in the earliest ages; and to the general elevation of these older heights with the rest of the area, the position of the great line of Himalayan peaks was attributed, rather than to any especial energy of upheaval along that particular line in any of the later changes. Until the commencement of the Tertiary period we can say little of the state of the area, but then the nummulitic sea may be traced extending across the greater part of Southern Asia, through Asia Minor, Persia, and Afghanistan, and along the foot of the Himalaya, even to the Kahasiya hills, east of Bengal. The mountain-area, above the level of the sea was at this time probably restricted, for the traces of the ocean of that period have been found in Tibet, at a place where a pass now rises to a height of 16,500 feet. At the close of the nummulitic period, a general elevation seems to have taken place over all Southern Asia, and the table-land of Afghanistan was finally raised above the Southern sea, which, however, still swept over the whole of the existing plains of India; while the Caspian probably extended over all the steppes of Western Turkistan, to the foot of the Hindu-kùsh. To the north of the Himalayas, the Northern sea appears to have covered the table-land of Tibet, occupying long fiords or estuaries between the mountain-ranges, which had already commenced to rise.

By the middle of the Tertiary epoch the Siwalik Range had been brought into its existing position, the whole of the great Tibeto-himalayan system had been greatly raised, and the upward motion extended to the basin of the Caspian, which sea was now separated from that of Aral. But the Indian Ocean probably still covered the northern plains

of India, nor was it until long after that it retired to its existing shores. Finally, the evidences of the diminution of the Himalayan glaciers in the existing epoch, which are to be met with at all parts of the chain, were attributed to the change of climate that must have accompanied this last change of the marine area, by which the conditions, becoming more *continental*, the summer temperatures would be raised, and the fall of snow diminished. This, therefore, might have had the effect of causing the snow-line and glaciers to recede, although the actual elevation of the mountains had been increased about 1000 feet.

4. *Eruption of Vesuvius*, (from letters in London Daily News, dated Naples, May 5 and 10, 1855.)—*Saturday*, *May* 5.—Having purchased our torches at Resina, we turned out of the high road into the comparatively narrow and heavy route which begins the ascent. It is formed of loose volcanic dust and pulverized lava; and hard work it is indeed for the weary horses to get along. Ours acted most prudently by refusing to advance, so that, dismounting, we took to our legs. A woman might have gone up alone, so dense were the crowds either coming or going; for be it known that apart from curiosity many felt not a little relief at the eruption, as though it had saved them from the disasters of an earthquake, and were full therefore of joyousness. As we got close under the mountain we experienced something like disappointment, for the elevation on which the Hermitage stands hid from our view the fire and smoke and the streams of lava which even from Naples formed so magnificent a spectacle. As we got higher and higher the glare of light reflected on the sky became visible, and by the time we had got to the Hermitage the grandeur of the scene began to open upon us. Pushing on from this point through cicerones and donkeys, horses, carriages and christians, as bipeds are styled in Naples, we traversed for some distance the road which leads to the cone, when, turning sharp off to the left, we arrived at a point on the stream of lava where it forms a cascade. This is about a mile or a mile and a half from its source; and here a vast crowd was assembled, as though it afforded the grandest *colpo d'occhio*. To tell the truth, your correspondent labors to write coolly, for such was the magnificence of the scene that were one to trust too much to his feelings he would be hurried into what might appear exaggeration. The lava on which we stood was yesterday a boiling, moving stream; it was still hot to our feet, and taking up the loose pieces of scoria the glowing fire was clearly perceptible beneath; we lit our cigars at it, and played all kinds of practical jokes; and, jumping over the fiery fissures, stood by the side of the bed of moving lava. Imagine thousands of tuns of coke carted out together, and rolling mass over mass, and some idea may be formed of the noise—not that it was loud, but a wide spread incessant sh–sh–sh–sh–like water over pebbles. When a great accumulation of materials had been formed at the edge of the precipice, the outer blackened crust broke up, and rolling over into the abyss below, bounded from rock to rock down among the chestnut trees, which writhed and flamed up, and then fell over. The immense body of heat and light which then burst forth nearly scorched and blinded us, and instinctively we held up our hands to ward it off. The breadth of the stream in this direction is 100 palms, says the Neapolitan journal; from my observation, I should say nearer 200 palms.

Of course all calculation must be mere guess-work, as who can measure a fiery flood? I never witnessed such mighty results of power apparently so unconnected with any cause. It produced the same impression upon me that the Toledo or Strand might do were either to take it into its head to walk. There was a solid plain which we might have crossed some eight and forty hours before now going full drive over a precipice some thirty or forty feet deep, and then stealing onward, as it now is, through chestnut groves and vineyards and villages, and threatening places of some consideration. Above the precipice the stream—or rather two streams, which are united at the cataract—flows through a plain in a serpentine form, and following back its course we arrived at the foot of the cone. Half way up, we came upon the first of seven mouths, all of which threw out either lava or stones, or both. Those which threw out lava bubbled and gurgled over, while those which made a greater effort and threw out stones, kept up an incessant noise, as that of a distant, heavy cannonade. What a foreground was this! Behind these in the distance the background was formed of heavy masses of lurid clouds, showing off by a strong contrast the vivid fires in front. The lava flowed down the sides of the mountain in waves of fire, and rolling through the valley we had just passed, precipitated itself into the gulf above which we stood at first. Of course we ladled up some liquid lava and fixed some coppers in it, and then were glad to move off. The wind occasionally shifted, and with it that curtain of lurid clouds. Our guide warned us to be off on penalty of sharing the fate of Pliny, a consummation earnestly to be avoided, we thought, however great our respect for the old Roman. Since that evening the seven mouths have all resolved themselves into one. The lava still continues to pour down the mountain in the direction of San Sebastiano, Pollena and Massa di Somme, being the same direction it took in 1822. It has already occupied the bed of a river, destroyed the plantations, and is threatening still greater injury. The Marchesa St. Angelo, who has property in that direction, has been removing his furniture from his villa as a precautionary measure. In short, this eruption, though not so effective a spectacle at a distance as the last, is far more imposing when viewed close, and threatens to be much more destructive.

Thursday, May 10.—The lava has now advanced ten miles from its source and is doing terrible damage. I have before me the report of Cozzolino as to the latest changes which have taken place about the cone. Just at the base of it a lake of fire has been formed which looks like a red sea in an undulatory state. In the very center of this has opened another crater, which is throwing out red-hot stones. On the morning of the 7th the crater at the very summit fired, as it were, two heavy cannonades; and after sending forth lightning, flames and stones, broke up altogether. In the middle of the cone ten craters have been formed, and from these the lava pours forth like a river and runs on the side of the Cavallo as far as the Minatore. Here four other craters have been formed, which resemble gigantic exhibitions of fireworks. The whole of the summit is therefore like a sponge and must inevitably fall in. The thin crust trembles under your feet. You may see the stones dance with the tremulous movement; the part immediately round the crater

looks like the sides of a heated copper boiler. Such is a true statement of what is going on at the summit. There are reports of an opening toward Pompeii, which is not unlikely, and of another toward Resina, but I have not been up for some days, as the danger is now very great. Before I write again I shall make the attempt.—Last night I went to the scene of most stirring interest after an interval of two days. The whole length of this usually quiet road was like a fair, and such was the throng of carriages which were moving on in three lines that it was with difficulty we ever arrived at our destination. As we approached the menaced neighborhood the inhabitants were removing their goods, and on a bridge in the middle of the little township of Cercolo (through which in the winter time thunders down from the summit of Vesuvius one of those mountain rivers so well known in Italy) stood a company of sappers. Creeping under the solid handsome bridge into the bed of the river, we went up in face of the lava, which, was now coming rapidly down. Here again were sappers, raising mounds on either side to divert the ruin from some private grounds and keep the lava in one straight course. The smoke which rose over the heads of the multitudes told us we were close on the spot, and climbing up the bank and walking along the top we looked down on this mighty mass of fire. How changed the neighborhood in two days? Where I walked on Sunday night was now a sea of fire. The side road by which I had come down into the main stream from Pollena and Massa di Somme was now full of blackened coke. The houses on the borders of the village had fallen—in one thirty poor people lived; a small chapel was swallowed up, a gentleman's villa, and a sad extent of vineyard and garden ground. On the other side of the great lava bed another stream was branching of to San Sebastiano. We had hoped to cross it and ascended to the cascade again, but it was no longer possible; for as one says speaking of a marshy country in the winter, the lava was out. The fire here had begun to enter the burial-ground of the little town, but was diverted from its course by a wall. On the opposite side of the stream were the king and all the royal family. The banks on either side were thronged with curious and anxious multitudes, whose faces were lighted up with the blaze of hundreds of torches and with the more resplendent light of the rapidly-descending lava. Since the morning it had moved a mile. It was like a vast river of glowing coke. As it moved on, the tens of thousands of lumps rolled and tumbled one over the other, crackling and grinding and grating; and when from the very face of it a large lump fell off, the appearance was that of an iron furnace when the iron is being drawn. To make the resemblance more complete, at such times men darted forward with long poles taken from the neighboring vineyards, and pulled out great masses of lava in which they imbedded money for sale. What struck me at first, and still strikes me as the most majestic feature in the whole scene, is the slow, silent, irresistible motion of that fiery flood. Active almighty power without an effort! Sweeping everything before it, overcoming every obstacle, growing up against intervening walls or houses, and devouring them bodily, and then marching on in the same silent, unrelenting, irresistible manner as before. There was a spot beneath my feet where a fall of mason work had been built to break the violence of the winter

floods: to this spot all eyes were directed. The fiery river would fall over it in an hour; as yet it was distant from it seventy yards perhaps. Gradually it rose in height and swelled out its vast proportions, and then vast masses fell off and rolled forward; then it swelled again as fresh matter came pressing down behind, and so it broke, and on it rolled again and again till it had arrived at the very edge. There was a general buzz and murmur of voices. The royal family stood opposite to me intermingled with the crowd looking on with intense anxiety. At last it broke, not hurriedly, still with a certain show of majesty. At first a few small lumps fell down; then poured over a pure liquid of metal like thick treacle, clinging sometimes mass to mass, from its glutinous character, and last of all tumbled over gigantic lumps of scoria. Then on it moved once more in its silent regular course, swelling up and spreading over the vineyards on either side; and now there was a rush for the road which traverses this lava-bed. Houses and the bridge bordered the road, the carriages had all been ordered off, and the bridge was being broken down—we were cut off completely. The sentinels would not let us pass, and struck us and drove us back; but we forced our way, and then found too surely that it was impossible to get on. The bridge was half demolished, and by the light of the torches we could see the soldiers above working away with the pick and the axe. We had therefore to retrace our steps, and making a long circuit through the open country and over walls, came round to the top of the bridge. "Run" said the sentinels, "or you will be too late." We crossed the narrow parapet which was still remaining, and soon afterward down went the whole fabric. In this way it is hoped that the lava will be diverted from the townships of St. Sebastiano, Massa di Somme and Pollena, which stand on either side and have as yet only suffered partially. Cercolo, through which, however, the stream is rolling, will be sacrificed. The expectation is that the lava, should the eruption continue, will flow down to the Ponte Maddaloni and into the sea. So grand and so destructive an eruption has not been known for many years, and even now we cannot tell how or when it will terminate. The mountain is literally seamed with lava and many fear a violent explosion as the final scene of the tragedy.

5. *A Manual of Elementary Geology*, or The Ancient Changes of the Earth and its Inhabitants as illustrated by geological records; by Sir CHARLES LYELL, M.A., F.R.S., &c. 5th edit. greatly enlarged, and illustrated with 750 woodcuts. Boston, Little, Brown & Co. 1855.—This new edition of "Lyell's Elementary Geology" has received very large alterations and additions, much new matter being introduced, both from the extended investigations of the author, and the various researches of other geologists. It is the ablest and fullest work of the kind in the English language.

The edition issued by Messrs. Little, Brown & Co., is the English edition, and not a reprint, having therefore all the perfections of the original work in the cuts and letter press.

6. *Fossils of South Carolina;* by Prof. M. TUOMEY and Prof. F. S. HOLMES, 4to. Charleston, S. C.—No. 2 of this elegant work has just been issued.

7. *Naturgeschichte der Vulcane und der damit in Verbindung stehenden Erscheinungen*, von Dr. GEORGE LANDGREBE; Mitglieder mehrerer gelehrten Gesellschaften. 2 vols., 8vo. Gotha, 1855.—Dr. Landgrebe has given in this work condensed accounts of all the volcanoes of the world in the first volume, and general views on their distribution, the nature of volcanic rocks and other topics in the second. He has overlooked the recent accounts of the Hawaian volcanoes in this Journal, and also the Geological Report by J. D. Dana, in which a large part is devoted to the subject of volcanic islands and phenomena.

8. *Lehrbuch der chemischen und physikalischen Geologie*, von Dr. GUSTAV BISCHOF. Bonn. Part 7 of this excellent work on Chemical Geology, has recently been issued.

9. *Uebersicht der Resultate mineralogischer Forschungen* im Jahre 1853, von Dr. ADOLF KENNGOTT. Leipzig, 1855. T. O. Weigel.—This is another issue of Dr. Kenngott's Mineralogical Annual, containing abstracts of all papers published on Mineralogy and related topics during the year 1853. It extends to 174 pages quarto. We shall return to it again in a future number of this Journal.

10. *Versuch einer Monographie des Borazites;* Eine fassliche angewandte Darstellung des jetzigen Standes der Krystallologie und ihrer neuesten Richtung: Ein Beitrag zur Geschichte dieser Wissenchaft und zur Kenntniss der Steinsalz-Lagerstätten und ihrer Bildung; von G. H. OTTO VOLGER, 244 pp., 8vo. Hannover, 1855, C. Rümpler.

11. *Materialen zur Mineralogie Russlands*, von NIKOLAI VON KOKSCHAROW, Berg-Ingenieur, Mitglied der R. K. Mineralogischen Geselschaft zu St. Petersburg, und des Naturforschenden Vereins zu Moskau, Lief. 6, 7, 8, 9, 10, 11, 12. 8vo, with plates in 4to. St. Petersburg, 1854.—M. Kokscharov, in these numbers continues his excellent work on Russian Mineralogy. A large number of new forms of crystals are carefully figured in the 4to plates. With part 9 the second volume of the work commences. We shall make citations from the work at another time.

12. *Die geologische Uebersichtskarte des mittleren Theiles von Süd-Amerika*, von FRANZ FOETTERLE; mit einem Vorworte von W. Haidinger. 22 pp., 8vo. Wien, 1854.—The geological map of South America accompanying this pamphlet is nearly two feet square. It is colored to represent the geological formations, and combines the results of the observations of various travellers. The details are more fully and exactly given with respect to Brazil, as stated in its title.

III. BOTANY AND ZOOLOGY.

1. "*Poetry of the Vegetable World: a Popular Exposition of the Science of Botany and its relations to Man;* by M. J. SCHLEIDEN, M.D., Professor of Botany in the University of Jena. Illustrated with engravings. First American, from the London Edition of Henfrey. Edited by ALPHONSO WOOD, M.A." Cincinnati and New York, 1853.—The first and principal remark we are called to make upon this book is that it appears under a false title. Prof. Schleiden, at the solicitation of friends, committed to the press, in 1847, a discursive course of popular lectures upon the *Plant and its Life.* His interesting and very

characteristic volume, entitled "*Die Pflanze und ihr Leben; Populäre Vorträge*, was admirably translated in England by Mr. Henfrey, and published by Baillière in 1848, under the English title of "The Plant; a Biography; in a series of Popular Lectures." The English publisher has beautifully reproduced the plates and the vignettes; and Mr. Henfrey's known ability is a guarantee that the text is faithfully rendered. In causing the book to be reprinted in this country, where it had already become pretty well known, Mr. Wood has taken what we must call the unwarrantable liberty of altering the title, as above.

What was the object of the change is more than we can imagine. Perhaps the factitious title was thought to be a more taking one than the real. So, indeed, it lately proved in the case of a friend of ours, who was *taken in* by it. Having purchased the new book by its title, he found, to his chagrin, that it was only a reprint (with the pretty vignettes and engraved frontispiece left out) of "The Plant," translated by Henfrey, a copy of which had long adorned his library. This mishap makes us regret that we had neglected to indicate this danger when the American reprint made its appearance in such a guise. We fully absolve Professor Wood from any equivocal intention in the matter; but taking liberties with names and title-pages is neither proper nor safe. The omission of the vignettes at the head of each lecture and of the engraved frontispiece might have been excused if the fact had been mentioned. The paragraph which relates to them in the author's introduction is duly given, as is a reference or two in the body of the work; but the pictures themselves have silently vanished; and some of the notes referring to them are mutilated so that the innocent reader would not detect the omission. So also Mr. Henfrey's name has almost vanished from the title page of his own literary production. We likewise miss Henfrey's sensible advertisement, stating that he had confined himself to the simple rendering of Schleiden's language into English, retaining "the peculiar character of the original," and leaving its faults and its beauties,—equally characteristic of the able but excentric author—to speak for themselves. The author himself, in his introduction, takes special pains to place this volume before the public in the proper light, namely, as a course of popular lectures, cast in a mould befitting for oral delivery, but not to be judged by the rules applicable to a treatise or a series of essays. Nevertheless, for some unaccountable reason, the American editor has thought fit to change this point of view as much as possible, by sedulously substituting the word '*Essays*' for '*Lectures*' throughout the Introduction, and by transforming the twelve 'Lectures' into 'Chapters:' while the word 'lecture' is carefully weeded out wherever it occurred in the discourses, and chapter awkwardly substituted for it. Turning to his preface, we find no explanation of this interference beyond the remark that: "In bringing this volume before the American public, we have *availed ourselves* of the English translation of Arthur Henfrey, F.L.S. The only objections to this excellent version which have appeared obvious to us were such as resulted from the translator's fidelity. In endeavoring to preserve, by a literal rendering, the true spirit and the glowing style of the original, he has given us occasionally sentences too long and involved, and sometimes too much encumbered with epithets to suit the genius of our language. In such

cases, we have frequently ventured to substitute a more simple version of our own." If what we have noticed are specimens of the substitutions referred to, we can only say that the objection of *fidelity* to the language and *true spirit* of the original is removed by the American editor as far as possible under the circumstances; but the *simplicity* of the new version is more apparent to us than its propriety or taste. A. G.

2. *De Vriese and Harting: Monographie des Marattiacées.* Leyden and Dusseldorf. 1853. Elephant 4to, pp. 60, with 9 plates.—The systematic part of this work is by Prof. De Vriese of Leyden. The researches upon the anatomy and organogeny of the genus *Angiopteris*, followed by considerations upon the structure of Ferns in general, are by Prof. Harting of Utrecht. Both parts possess much interest for botanists. The number of species appears to be unduly multiplied. To one of de Vriese's new species from Luzon must belong Mr. Brackenridge's *Angiopteris attenuata*, of the Ferns of the U. S. Exploring Expedition,—a volume long since printed, but the publication of which is unaccountably delayed. A. G.

3. *Pritzel: Iconum Botanicarum Index Locupletissimus.* Berlin. 1854. pp. 1183, imp. 8vo.—A most useful volume, containing references to 80,000 botanical figures; the names arranged in alphabetical order, in double colums, closely printed, in very neat style. The price of the book is very moderate. A. G.

4. *Botany of the Southern States.* In two parts. I. Structural and Physiological Botany and Vegetable Products. II. Descriptions of Southern Plants, arranged on the Natural System, preceded by a Linnæan and Dichotomous Analysis. By Prof. John Darby, A.M. New York: Barnes & Co. 1855. pp. 612, 12mo.—Prof. Darby is a distinguished teacher, and he has devoted much pains and study to the preparation of the present work, especially to the structural and physiological part of it. In this department he has been an original observer; while in Systematic Botany,—which has many more votaries in this country, pursuing it to a certain extent,—he would probably claim to have a good general, but no very profound acquaintance. The book is excellently planned, and is well adapted for its purpose,—that of a text-book for the Colleges and High Schools of the Southern States. While we cordially commend it in this view, we are free to say that a cursory perusal has indicated not a few points which are open to criticism. Most of them, at least in the structural part, however, are cases in which views once maintained by writers of authority, or even still current, are adopted long after they have ceased to be tenable. The remark may also apply to some original suggestions, such as that of the use or final cause of spiral vessels in the vegetable economy, and that on the structure of the monocotyledonous stem as compared with the dicotyledonous. An inspection of any herbaceous stems of these two classes of plants suffices to show that they belong to two distinct types, which cannot be reduced to one in the manner that Professor Darby supposes, whatever view be taken on the vexed question of endogenous growth. It is now well understood that the name of Endogen is not a very happy one; yet the structure of such stems is none the less distinct from the exogenous structure. On Prof. Darby's theory, if we

rightly understand it, the stem of a *Cycas* or a *Cereus* should exhibit the endogenous, and a corn-stalk or asparagus-shoot the exogenous, structure. But this is not the place to discuss the question.

The vitality of an erroneous statement is truly wonderful. A century ago Adanson published his account of the great Baobab trees of Senegal and Cape Verde, which account was corrupted by one or more popular writers about thirty years ago, in such a way as totally to mislead authors of high authority, such as the De Candolle's, and to give general currency to an estimate of the longevity of these trees which was totally unwarranted by the facts as narrated by Adanson. This falsification was pointed out eleven years ago, by the present writer (in the North American Review for July 1844): nevertheless the current statement has been reiterated ever since, in various works, of considerable authority or pretension. Prof. Darby has now repeated it with an addition which caps the climax of absurdity. He says,—doubtless copying from some other work, we know not what one:—

"A remarkable case of the deposition of external layers of dicotyledonous stems is related of the Baobab-tree (*Adansonia digitata*) of the Cape de Verde Islands. In the year 1400 Grew cut his name on two of these trees, and in 1749 (349 years afterwards) Adanson examined the same trees, and found the names with more than 300 layers of wood deposited over them." (p. 31.)

We had long ago shown that Adanson never pretended to have found any such thing, and that the current account asserting this was founded upon some strange mistatement or pure fiction. But in the present narration, *Grew*, the Vegetable Anatomist, who flourished in the latter part of the seventeenth century, and who perhaps was never out of England in his life, is said to have cut his name upon two Baobab-trees at the Cape de Verde Islands in the year 1400!

The old notion of *spongioles* at the extremities of roots,—organs which have no existence as described—is adopted on p. 39: but in the description they are confounded with the root-hairs, which are not the *spongioles* of De Candolle, nor are they "composed of lax cellular tissue," being only definite and free portions of superficial cells prolonged into tubes.

So far from the origin of adventitious buds having "completely eluded the research of philosophers," no part of organogeny is better known. The facts of the case are to be found, however, not in the speculations of Duhamel and Knight, or the assumption of Lindley (that their origin is connected with an anastomosis of woody fibre), but in the neat microscopical researches of Trécul and other modern observers.

The statement that the leaves of Pines, &c., "do not form a single spire, but a compound one, consisting of three or four spires running parallel to each other," shows that the writer has not discriminated between the real spiral on which the leaves are arranged, and the apparent or secondary spirals which necessarily become most conspicuous when the leaves are approximated. A moment's attention to the subject would enable Prof. Darby to express the elementary facts of *phyllotaxis* both correctly and clearly.

The fall of the leaf by an articulation has surely been much better explained by investigations of the facts of the case, by no means of

very recent date, than by Lindley's crude and imperfect hypothesis, which is most satisfactory to the author. For the want of a notice of *errata* (which are numerous, owing to the printing of the work at a great distance from the author's residence) we cannot ascertain what species is intended, on p. 60, by *Magnolia heterophylla*.

But what surprises us most is to find, now and then, in the course of a chapter which is on the whole clear and satisfactory, some gross absurdity like the following:

"Raspail asserts that the pollen is a production of the internal surface of cells within the theca, to which the grains are attached by a funicle. This is denied by other botanists." (p. 78.)

This is all that is given on the subject of the formation of pollen,—a subject on which perhaps it was not necessary to say anything in so elementary a treatise; but whatever is stated should have some resemblance to or compatibility with what is true, or what is or has been generally thought to be true. Whereas this statement is worse than that of the writer of an introduction to Astronomy would be who informed his pupils that: Ptolemy asserted that the sun and the other heavenly luminaries revolve around the earth: but this is denied by other astronomers:—inasmuch as Ptolemy was an authority in his day, and his view was long received, while Raspail's never was, the entire absurdity of it having been made as patent before Raspail wrote as it is now; and the formation of pollen is fully explained in most introductory works.

The crude, but long current notion that a young stem bends towards the light because "the side next the light deposits its carbon, and becomes firmer, and is contracted, while the other side remains turgid with unassimilated fluid," it is not so surprising that our author should continue to adopt. Yet, as he has evidently given particular attention to physiological questions, he ought to have known, before the year 1855, that this hypothesis has been demonstratively proved to have no foundation, as he might have learned from the pages of this Journal, and from editions of the principal text-books now two or three years old.

In respect to so large and so common an embryo as the Horse-chestnut, it is curious to read: "there is apparently but one cotyledon. Prof. Lindley says, that, by dissection, there is a slit which indicates the division between the two bases of a pair of opposite confluent cotyledons." It is hardly worth while to bring in the dictum of a distinguished foreign professor to vouch for a fact which the youngest of Prof. Darby's pupils could verify in a moment, by slicing a horse-chestnut, or observe directly when these large seeds germinate in the spring. If, however, these pupils proceed to verify, upon a grain of corn, or the seed of most other common Endogens, the Professor's definition of the monocotyledonous embryo; viz, that "it is a homogeneous, cylindrical body, tapering at both ends, with no distinction of radicle, plumula or cotyledon," they will naturally be not a little puzzled by the total discrepancy between the definition and the fact.

Our author gives an account of the formation of the embryo on the Schleidenian hypothesis alone, thus impliedly by adopting that view; but without making the least allusion to the mass of observations, of which he cannot be wholly ignorant, which have negatived every es-

sential point of the hypothesis, and every presumption that may have existed in its favor.

The only additional remark for which we have room relates to a question of taste. We are confident that our excellent friend, on being made aware of the idea which his paragraph, No. 241, conveys solely through the unfortunate choice of a phrase, will in another edition no longer speak of our Savior as *yielding His assent* to the sentiment of a quotation from Young's Night Thoughts. A. G.

5. *Wheat from Ægilops.*—The announcement by Prof. Dunal, of Montpellier, two or three years ago, that M. Fabre, of that vicinity, had converted *Ægilops triticoides* into wheat, by cultivation for several generations, excited a lively sensation, which has not yet subsided. Prof. Dunal appears to have satisfied himself that *Ægilops triticoides*, or rather its ancestor, *Æ. ovata*,—a common grass on the southern coast of France,—is the original of wheat, and Prof. Lindley has adopted this opinion. This, if true, would be the only instance in which any one of the staple cereal grains has been identified with a wild original. Dunal's published account has called forth many detailed discussions, in which various views have been maintained: but at length a few simple experiments of M. Godron of Besançon appear to have disposed of the question. M. Godron's paper is published in the *Annales des Sciences Naturelles*, vol. ii. for 1854, No. 4. He remarks, in the first place, that the so-called species, *Ægilops triticoides*, is only of sparse and occasional occurrence; that it is seldom if ever found except in the vicinity of wheat fields, and in the districts where *Æ. ovata* abounds as a wild plant; that intermediate states between *Æ. ovata* and *Æ. triticoides* do not occur, as they are apt to do between races or varieties of any species; but that *Æ. triticoides* itself varies in certain respects and according to the kind of wheat which is cultivated in the neighborhood; and, finally, that the wild *Æ. triticoides* usually produces very little seed. From these considerations he was naturally led to suspect *Æ. triticoides* to be a hybrid, resulting from the accidental fecundation of *Æ. ovata* by the pollen of wheat. And this conclusion he has verified by direct experiment; that is, he has raised *Æ. triticoides* from seeds produced by impregnating the ovaries of *Æ. ovata* by wheat pollen. At the same time, and in the same manner, M. Godron produced a new and analogous hybrid by impregnating *Ægilops triaristata* with the pollen of common wheat; as well as another by impregnating *Æ. ovata* with the pollen of *Bearded Spelt* (*Triticum Spelta, barbatum*). It seems, therefore, most probable that M. Fabre's Ægilops-wheat owed its origin to the accidental fertilization of the *Æ. triticoides* experimented with—itself a hybrid between wheat and *Æ. ovata*—by the pollen of its male parent, wafted from adjacent wheat-fields; the cross-breed returning to the male type in the usual manner under such circumstances.

This evidence, however, does not convince Dr. Lindley; who objects that M. Godron and others have not explained what the origin of wheat has been, if it is not a domesticated condition of *Ægilops*:—an undertaking which we must say is by no means incumbent upon M. Godron, who had accomplished his object when he has shown, as he has clearly done, that M. Fabre's famous experiments do not prove Ægilops to be

the original of wheat;—although in his opinion the two should be ranked in the same genus. Can Dr. Lindley indicate the wild original of Maize, Rye, Barley, or Rice? And does the fact that the originals (if they indeed exist) are unknown, render it any more likely that these cereal grains are the progeny, in altered guise, of some other known Gramineæ? A. G.

6. *Botanical Necrology and Intelligence.*—We have to record the death of two distinguished botanists, viz, Dr. MOLKENBOER, of Leyden, an accomplished muscologist, the author, in conjunction with Dr. Dozy, of the *Bryologia Javanica*, a work recently commenced, and of which five fascicles have been published. The surviving author, Dr. Dozy, informs us that he shall continue this excellent work, notwithstanding this great calamity, if it shall meet with a sale sufficient to defray the expenses of production. Dr. Molkenboer died in September last, at the early age of 38, deeply regretted by a wide circle of friends.

C. A. MEYER, long a distinguished member of the Imperial Academy of St. Petersburg, and, since the removal of the late Dr. Fischer, the Director of the St. Petersburg Botanic Garden, of late years the principal representative of botanical science at the Russian capital, died on the 25th of February last, at the age of 59 years. It is said that the important position vacated by the death of Dr. Meyer has been offered to Prof. Griesbach of Göttingen, and that he will probably accept it.

A. G.

7. *Description of a New Genus of Crinoidea;* by LUNSFORD P. YANDELL, M.D.*—In a paper on the geology of Kentucky by Dr. Shumard and myself, published in 1847, the figures of several new Crinoidea were given, and among the rest that of the remarkable individual which I am about to describe. It was found in Grayson County, Ky., near some mineral springs which have attracted considerable notice on account of their medicinal qualities. A very slight examination of this Crinoid, I think, will satisfy any palæontologist that it must form a new genus.

ACROCRINUS. (*ακρος*, *extremus*, and *κρινον*, *lilium*.)—Body goblet-shaped, constituted of about 16 series of plates generally hexagonal, and increasing in size from the base to the summit, each series including from 20 to 25 pieces, the last supporting 5 large arm-bearing plates, which give origin to 20 rays composed of a double row of tentaculated joints; pelvis undivided? large, circular, saucer-shaped; column round, consisting of thin, serrated joints gradually expanding towards the base of the cup.

The specimens of this crinoid yet discovered have not enabled us to determine with certainty the position or character of the oral orifice, or the number of pieces composing the pelvis. Its claims to form a new genus rest upon the extraordinary number of series of plates constituting the body from the pelvis to the arms, and the increase of the plates in size as they ascend. To this may probaby be added, an undivided pelvis, in which it agrees with no genus but Platycrinus, which is separated from Acrocrinus as widely as possible by the number, size, and arrangement of its plates.

* Communicated for this Journal.

Acrocrinus Shumardi.—But one species of this crinoid has been found. In this, the plates are thin, smooth, and without ornament. The body of the largest specimen from the pelvis to the rays is two inches in length. Alimentary canal round and diminutive.

The figure represents the specimen described, showing the body flattened and somewhat mutilated, with one ray and a part of the column attached. An interesting feature in this specimen is the presence of a univalve mollusc (*Capulus*) on the summit of the crinoid, in a position to have been grasped by the arms of the living animal. In the paper to which reference has been made, we ventured the suggestion that the encrinite was probably devouring the shell-fish at the time when it perished, but the disproportionate size of the prey to the eater caused a doubt as to the correctness of this opinion. Since that time however we have found

numerous specimens of encrinites with univalve mollusca in a similar position, and now no longer question the truth of our first conjecture.

Locality.—Carboniferous limestone, Grayson County, Ky., associated with Archimedes, Pentremites florealis, Poteriocrinus spinosus, *Owen* and *Shumard*, and numerous undescribed Crinoides.

IV. Astronomy.

1. *Elements of Dien's Comet*, (Compt. Ren., t. 40, p. 636.)—The following parabolic elements of the comet discovered Jan. 14, by Mr. Dien of the Paris Observatory were computed by Mr. Oudemans from the observation at Berlin of Jan. 15, Kremsmunster Jan. 28, and Leyden Feb. 17.

Perihelion passage 1854, Dec. 16·074, M. T. Greenwich.

Long. perihelion,	165° 52′ 47″	Mn. Eqnx.
" asc. node,	238 19 8	1855, Jan. 1
Inclination,	14 10 57	
Log. q.,	0·13588	

Motion direct.

2. *New Planet*, (Astron. Jour., No. 84.)—Mr. Chacornac of the Paris Observatory discovered a new planet on the 6th of April, in the 13th hour of right ascension, which has been named *Circe*. It is of the eleventh magnitude, and if an asteroid, forms the thirty-fourth of the group. A set of elements for this planet has been computed by Lesser from the following observations:

1855,	h.	m.	s.							
April 6,	14	44	14·0	204°	57′	5″·3	−7°	27′	58″·1	Paris.
13,	10	2	54·1	203	36	35 ·4	6	38	57 ·2	Vienna, Berlin.
20,	10	49	20·7	202	12	53 ·3	−5	49	36 ·6	Berlin.

1855, April 20·4509 M. T. Berlin.

Mean anomaly,	38° 48′ 53″·0	
Long. perihelion,	157 51 19 ·7	Mn. Eqnx.
" asc. node,	184 1 45 ·3	1855·0
Angle of excentricity,	6 34 5 ·0	
Inclination,	5 10 10 ·5	
Log. semi-axis major,	0·426961	
" Mn. daily motion,	2·909565	

3. *New Planet*, (Astron. Jour., No. 84.)—A thirty-fifth member of the group of asteroids was detected by Dr. Luther in Bilk, April 19, which has been named *Leucothea*. The following positions were obtained by Professor Rümker:

	h.	m.	s.	Planet's R. A.			Planet's Dec.			
1855, April 21	10	13	18·6	180°	57′	46″·1	−5°	10′	16″·7	19
22	10	40	13·0		49	21 ·3	−5	10	45	2

4. *New Comet*, (Astron. Journ., No., 84).—Dr. Schweitzer discovered a telescopic comet at Moscow on the 11th of April, and observed it as follows:

1855 April 11,	184° 40′	−17° 20′
15,	182 20	−13 40

5. *Cause of the Zodiacal Light;* by Rev. GEORGE JONES, Chaplain U. S. N., (Astron. Journ., No. 84.)—In my recent cruise in the U. S. steam-frigate Mississippi, chiefly in the China and Japan Seas, but taking us also around the globe, I had excellent opportunities for observation of the Zodiacal Light. This light, you know, appears to the best advantage within the tropics, where it stretches upward to a great elevation, and is a remarkable object; but it is also very desirable to observe it in high latitudes; and in this also I was favored, as our voyaging extended from 41° north to 53° south latitude, and in some instances our transitions, for weeks together, were very rapid, thus giving me opportunities for observing whether any parallel could be made or not.

I was also fortunate enough to be twice near the latitude of 23° 28′ north, when the sun was at the opposite solstice, in which position the observer has the ecliptic, at midnight, at right angles with his horizon, and bearing east and west. Whether the latter circumstance affected the result or not I cannot say; but I then had the extraordinary spectacle of the Zodiacal Light, simultaneously at both east and west horizons, from 11 to 1 o'olock, for several nights in succession.

In the first part of our cruise my observations were of a desultory character; but I soon began to see the necessity of great precision, and accordingly constructed star-charts from a celestial globe (a small but excellent one) that happened to be on board, which charts I afterwards had cut in wood at Canton, and thus I was furnished with materials for accurately recording all the changes of this phenomenon, not only in successive nights, but also in the successive hours of the same night. My rule was to draw on my chart the boundaries of the Zodiacal Light as exhibited among the stars, with such annotations as the case required; and then, again do the same after an interval of an hour or half-hour; and so to continue, generally as long as the boundaries could be made out reliably; then, if the morning admitted it, to resume the observations at the earliest possible hour, and so to proceed until the dawn. I have thus, in many instances, observations for every hour of the night.

At an early period I began to query whether the moon, when near its full, might not give a Zodiacal Light: and at last when I had gained more experience in observing, and in the peculiar character of this light, I was able to get, at different periods, fourteen reliable observations of what I think must be considered a Zodiacal Light produced by the moon. I have also two records of a distinct Zodiacal Light produced by the joint action of the sun and moon, i. e. at the hour when the moon, then near its first quartering, was about 65° above the western horizon; the reflection from the combined light of the sun and moon being sufficient to overpower the moonlight proper, and thus to produce a decided stream of light in the sky within the Zodiacal-Light boundaries. The latter of these observations was the more remarkable inasmuch as the moon was then *without* the boundary of this reflected light.

You will excuse my prolixity in stating these varieties of observation, for the conclusion from all the data in my possession is a startling one. It seems to me that these data can be explained only by the

supposition of a nebulous ring with the earth for its centre, and lying within the orbit of the moon. This conclusion seems to evolve itself, —1st, from the simultaneous midnight east and west observations, which preclude the possibility of a ring around the sun *within* the earth's orbit; 2d, from the great hourly lateral changes (often semi-hourly) in the boundaries of the light caused by the observer's change of place, in that time, as regards the ecliptic or axis of the Zodiacal Light, which lateral change in the light is too great to allow of our considering it at a distance of 170,000,000 of miles, as its lower end would be, near dawn, if it is a ring around the sun and beyond the earth's orbit; and 3d, from the moon's Zodiacal Light, if real, which I think it is. That it is a ring, the unbroken continuity of my observations satisfactorily determines. For more than two years I never failed to see this Light, evening and morning, when the moon and clouds did not interfere: and, except one evening, I have continuous records of this kind.

I could get no parallax; but, on the contrary, as we went south, the boundaries of the Zodiacal Light changed with us to the south among the stars; and so *vice versa*, towards the north, caused, doubtless, by the ring's presenting new portions of its wide reflecting surface to the sun's light.

V. Miscellaneous Intelligence.

1. *Contributions to Meteorology.—Mean results of Meteorological Observations made at St. Martin, Isle Jesus, Canada East*, (nine miles west of Montreal,) *for* 1854; by Charles Smallwood, M.D.—The geographical co-ordinates of the place are 45° 32′ N. Lat., and 73° 36′ W. Long. Height above the level of the sea, 118 feet.

Barometric Pressure.—The readings of the barometer are all corrected for capillarity, and reduced to 32° F. The whole of the means are obtained from three daily observations taken at 6 A. M., 2 P. M., and 10 P. M.

The mean height of the *barometer* in January was 29·516 inches, in February 29·520, in March 29·024, in April 29·440, in May 29·731, in June 29·814. in July 29·916, in August 29·910, in September 30·001, in October 29·949, in November 29·764, in December 29·540 inches. The highest reading was on the 23d of December, and indicated 30·954 inches; the lowest was in March, on the 16th day and was 28·607, inches; the yearly mean was 29·677 inches; the mean yearly range was equal to 1·907 inches. The *atmospheric wave* of November was marked by its usual fluctuations, the final trough terminated on the 25th day.

Thermometer.—The mean temperature of the air, by the *standard thermometer*, was in January 10° 92, in February 12° 20, in March 25° 84, in April 37° 75, in May 57°·17, in June 63° 80, in July 76° 20, in August 68° 31, in September 58° 01, in October 48° 40, in November 32° 99, in December 7° 35. The highest reading of the *maximum thermometer* was on the 20th of July and marked 100° 10, the lowest reading of the *minimum thermometer*, was on the 23d of December and was —36° 20 (below zero). The mean temperature of the quarterly periods was, Winter 13° 26, Spring 40° 25, Summer 69° 43, Autumn 46° 46. The *yearly mean* was 41° 57, and the mean yearly range 136° 30.

The greatest intensity of the sun's rays was in July, and indicated 140° 2, the least intensity was in December, and was 58° 0, and the lowest point of terrestrial radiations was −36° 9, (below zero) in December.

The mean humidity (saturation being 1·000) was, in January ·843, in February ·825, in March ·840, in April ·835, in May ·723, in June ·780, in July ·709, in August ·714, in September ·781, in October ·874, in November ·878, in December ·850. The yearly mean was ·804, which was *minus* ·021 of last year.

Rain fell on 80 days; it was raining 231 hours 16 minutes, and amounted to 40·505 inches, and was accompanied by thunder and lightning on 14 days. I observed no yellow matter in the rain, which fell this year. The amount of rain which fell in January was 1·067 inches, in February 0·150, in March 0·910, in April 7·886, in May 3·418, in June 8·384, in July 0·174, in August 2·265, in September 6·167, in October 4·844, in November 5·130, in December 0·110 inches.

Snow fell on 53 days, it was snowing 222 hours 6 minutes, and amounted to 97·45 inches on the surface. The monthly fall was as follows: in January the amount was 17·98 inches, in February 23·96, in March 28·61, in April 4·03, in October 3·10, in November 1·10, in December 18·67 inches. The first snow of the winter 1853–4 fell on the 15th day of October 1853, and the last fell on the 30th day of April 1854; the whole amount of snow which fell in the winter 1853–4, amounted to 116·81 inches. The river Jesus was frozen over on the 28th day of November. The last steamer left Montreal (on the St. Lawrence) on the 7th of December 1853; the first steamer arrived at Montreal on the 22d day of April 1854. The winter 1853–4 fairly set in on the 17th day of December.

The amount of evaporation was measured regularly from the 1st of April to the 31st of October, and amounted in April to 1·86 inches, in May 4·13 inches, in June 2·95 inches, in July 5·12 inches, in August 4·70 inches, in September 3·11 inches, in October 1·49 inches. This period includes what I consider could be taken with anything approaching to accuracy, owing to frosty weather. The greatest amount of evaporation I have observed here, was on the first six days of June and amounted to 1·00 inches. The month of July was remarkable for its dryness the amount of evaporation in the month was 5·12 inches, and the amount of rain which fell *amounted only* to 0·174 inches, the temperature was 8° 2, above that of last year. Very few birds were seen in the fields, owing to the scarcity of water, and vegetation suffered very much from the unusual drought. The mean humidity of the month was ·709, while that of the corresponding month of last year was ·727, and of 1853, ·845.

The most prevalent wind, during this year was the N. E. by E.; the least prevalent was the S. S. E. In the winter quarter the most prevalent wind was the N. E. by E. and the least S. In the spring quarter the most prevalent wind was the N. E. by E. and the least S. S. E. In the summer quarter the most prevalent wind was the S. W. by W. and the least so the E. In the autumn quarter the most prevalent wind was W. S. W. and the least E. The greatest velocity of the wind was on the 4th day of December, and was 40·17 miles per hour, the yearly

mean of the *maximum velocity* was 19·53 miles per hour, the yearly mean of the *minimum velocity* was 0·16 miles per hour. The quarterly means were as follows: winter maximum velocity 17·06, minimum velocity 0·12; spring maximum velocity 19·86, minimum velocity 0·21; summer maximum velocity 55·51, minimum velocity 0·00; autumn maximum velocity 13·80, minimum velocity 0·31 miles per hour.

Crows wintered in this place although the winter was very severe, wild geese, *Anser canadensis*, were first seen on the 20th day of April; swallows, *Hirudo rufa*, were first seen on the 12th of April; shad, *Alosa*, were first caught in this neighborhood on the 30th of May; fire-flies, *Lampyris corusca*, were seen on the 12th day of June; frogs, *Rana*, were first heard on the 2nd of May. The "*Rossignol*" (the harbinger of the Canadian spring) was said to be heard on the 16th of March, and snow-birds were last seen on the 27th of April.

The Aurora Borealis was visible on 50 nights as follows:

January 8th, 2.40 A. M. Faint auroral light.—23d, 10 P. M. Low auroral arch faint.—28th, 6.30 P. M. Dark segment at the horizon, arch distinct and bright, occasional streamers; a second arch was visible passing from west to east through the constellation "*Taurus*;" 6.55, the upper arch less distinct; 9.15, auroral light diffuse; 10.0, upper arch vanished, frequent and brilliant streamers to the zenith; 11.0, lower arch very bright and less diffuse.—29th, 8.0 P. M. Faint auroral light in the horizon; 9.0, idem, dark segment at the horizon; 10.0, idem. Aurora might have been seen on 11 nights this month.

February 10th, 10 P. M. Very faint auroral light.—11th, 4 A. M. Bright auroral arch at the horizon, frequent streamers.—16th, 9 P. M. Brilliant arch, dark segment at the horizon, auroral light of a bright, white color; 10 P. M., dark segment vanished.—24th, 7 P. M. Dark arch at the horizon, surmounted by a very bright arch of auroral light; 9 P. M., arch extended to near the zenith of a very pale color and extending from east to west.—27th, 10 P. M. Two distinct arches of auroral light, patches of auroral clouds in the west, occasional streamers of a green and yellow color. *Zodiacal light* very bright during the month. Aurora might have been seen on ten nights.

March 6th, 10 P. M. Faint auroral light at the horizon.—12th, 10 P. M. Lunar halo, diam. 35° 6′.—16th, 10 P. M. Heavy *cumulo-stratus* at the horizon, auroral light visible above them.—20th, 10 P. M. Very faint auroral light.—21st, 7.45 P. M. Auroral arch stretching from E. to W., and passing to the horizon forming the base of the *zodiacal light*, which is very bright and well defined; 10 P. M., very faint.—29th, 9 P. M. Patches of auroral clouds in W. et W. N. W., and E. None in the N., or if any, very faint.—30th, 10 P. M. Faint auroral light. Aurora might have been seen on 7 nights.

April 3d, 8 P. M. Comet visible.—6th, 8 P. M. Comet still visible.—10th, 9 P. M. Lunar halo, diam. 60°.—14th, 10 P. M. Very faint auroral light at the horizon.—15th, 10 P. M. Auroral arch 2° wide, stretching from the eastern to the western horizon, of a pale yellow color, passing through the constellations "*Cassiopeia*" and "*Taurus*."—21st, 9 P. M. Bright auroral light in the N.; 9.36, an arch of auroral light passing a little south of the zenith through "Leo," and stretching from E. to W., the eastern limit somewhat distorted; 9.40,

arch vanished; 10.0, auroral light still in the N.—23d, 10 P. M. Faint auroral light at the horizon. 24th, 10 P. M. Idem. Aurora might have been seen on 8 nights.

May 4th, 10 P. M. Faint aurora in the N.; dark segment underneath.—16th, 9.30 P. M. Patches of auroral clouds forming an imperfect arch in the zenith, faint auroral light at the horizon.—23d, 10 P. M. Faint auroral arch, dark segment underneath, frequent streamers from the eastern limit.—26th, 10 P. M. Faint auroral light to the horizon.—28th, 9.45 P. M. Very faint auroral arch in the zenith passing from E. to W., lasting only a few minutes. Aurora might have been seen on 15 nights.

June.—Aurora Borealis was visible on 2 nights, but very faint. Might have been seen on 13 nights.

July 3d, 11 P. M. Auroral arch of moderate brightness, frequent streamers.—5th, 10 P. M. Faint auroral light.—12th, 10 P. M. Idem and occasional streamers.—14th, 10 P. M. Faint light, dark segment underneath.—27th, 29th and 30th, 10 P. M. Very faint light at the horizon. Aurora might have been seen on 14 nights.

August 17th, 10 P. M, Auroral arch of moderate brightness at the horizon.—20th, 10 P. M. Auroral light faint, dark segment underneath.—25th, 10 P. M. Bright auroral light, frequent streamers. Aurora might have been seen on 15 nights.

Sept. 10th, 10 P. M. Faint aurora, frequent streamers, violet color. 12th, 10 P. M. Auroral light to the horizon, of moderate brightness.—13th, 10 P. M. Faint auroral light.—15th, 10 P. M. Idem.—17th, 10 P. M. Faint auroral light, dark segment underneath.—20th, 10 P. M. Two distinct arches of faint auroral light of about 3°; dark band between them of 8° broad.—21st, 10 P. M. Auroral light, dark segment underneath.—26th, P. M. Faint auroral light. Aurora might have been seen on 10 nights.

Oct. 18th, 10 P. M. Faint auroral light at the horizon.—25th, 10 P. M. Idem. Aurora might have been seen on 12 nights.

November.—Aurora borealis invisible this month. Might have been seen on 8 nights.

December.—Faint aurora borealis on the 8th, 18th and 22d. Might have been seen on 10 nights.—26th, 10 P. M. *Lunar halo*, 45° 6′.

Zodiacal light very bright this month.

Electrical state of the Atmosphere.—The atmosphere has daily afforded indications of electricity, but of less intensity than that of last year.

Ozonometer.—Observations of the amount of ozone are still registered twice daily, and as might be supposed from the dryness of the year, indicated a less amount than for the last year, for the presence of moisture in the atmosphere generally shows an increase.

St. Martin, Isle Jesus, C. E., January, 1855.

2. *Microscopes of American Manufacture.*—A few years since, only five or six, it was a thing impossible to obtain in the United States a microscope of any value of domestic manufacture. Thanks to the genius of Spencer and the Messrs. Grunow of New Haven, this is now quite possible. The merit of Mr. Spencer in this department of optical art, the clearness of definition, power of illumination, absence of color

and breadth of angular aperture, in his higher powers are well known and have been long since acknowledged—even by his European competitors. In the New York Crystal Palace Exhibition the first prize for Microscopes was awarded to Mr. Spencer, the second to Messrs. Grunow. Since that time the latter makers have steadily advanced in the excellence of all parts of their instruments. Their form of stand for steadiness, ease of management, simplicity and economy is unrivalled —and the moveable stage with universal motion in one plane by one lever, improved and admirably executed by them, is now universally adopted in the best American instruments. Their brass work is perfect in all respects. One of the most experienced microscopists in the United States writes as follows, of the optical value of their objectives: "I have tried their $\frac{1}{8}$th objective, and it compared so well with my Spencer $\frac{1}{8}$th that I consider them of equal value. I have also a $\frac{1}{4}$th inch objective made by them which works quite as well as the best English glasses. I need not tell you how much I rejoice that so worthy a man as Mr. Grunow has succeeded so well as an optician. His students microscope I have been familiar with for some time. It is a source of congratulation that our students of medicine can now obtain the means of pursuing microscopic studies at a reasonable cost, and without the vexatious delays we have experienced in importing European instruments."

The Inverted microscope invented by Dr. J. L. Smith, is built only by them and in a very superior style of workmanship. They also produce very perfect geodesic and surveying instruments, and are prepared to undertake any instrument of research or illustration required in refined physics or chemistry. We take pleasure in calling the attention of scientific men generally to the establishment of Messrs. Grunow, as adding a new and valuable resource to their means.

We have recently received a circular, from Wm. Buffhum & Son, makers of Microscopes and Telescopes at Milburne, Lake Co., Illinois.

3. *Melloni.*—An oversight occurs in M. Nicklès's obituary notice of the distinguished physicist, *Melloni*. He states that Melloni's "treatise, entitled, *La Thermochrosie*, was found among his papers, and published by his scientific friends." This can only refer to a second part of that interesting work. The first volume was published at Naples in the year 1850, and was well known to the physicists of this country during the author's life-time. The writer of this had the pleasure to receive a copy from Melloni a year ago.

4. *On the Application of Photography to Astronomical Observations;* by Sir John F. W. Herschel, (Proc. Astronom. Soc. in Athen., No. 1435.)—I consider it an object of very considerble importance to secure at some observatory, and indeed at more than one, in different localites, daily photographic representations of the sun, with a view to keep up a consecutive and perfectly faithful record of the history of the spots. So far as regards the general delineation of the whole disk, and the marking out on it, in reference to the parallel to the equinoctial passing through its centre, the places, sizes, and forms of the spots, there would need, I should imagine, no very powerful telescope—quite the contrary; but it should be equatorially mounted, and ought to have a clock motion in the parallel. The image to be impressed on the paper (or collodionized glass) should be formed not in the focus of the object-lens, but in that of the eye-lens, drawn out somewhat beyond the

proper situation for distinct vision (and always to the same invariable distance to insure an equally magnified image on each day). By this arrangement, a considerably magnified image of the sun, *and also of any system of wires* in the focus of the object-glass, may be thrown upon the "focussing-glass" of a camera-box adjusted to the eye-end of a telescope. By employing a system of spider-lines, parallel and perpendicular to the diurnal motion, and so disposed as to divide the field of vision into squares, say of 5′ in the side, the central one crossing the sun's centre (or rather, as liable to no uncertainty, one of them being a tangent to its lower or upper limb), the place of each spot on the surface is, *ipso facto*, mapped down in reference to the parallel and declination circle and its distance from the border, and its size measurable on a fixed scale. If large spots are to be photographed specially with a view to the delineation of their forms and changes, a pretty large object-glass will be required, and the whole affair will become a matter of much greater nicety; but for reading the daily history of the sun, I should imagine a 3-inch object-glass would be ample. The representations should, if possible, be taken daily, and the time carefully noted. As far as possible, they should be taken at the same hour each day; but in this climate, a clear interval, occurring when it may, had better be secured early in the day. Three or four observations in tropical climates, distant several hours in longitude (suppose 3, at 8^h distance in longitude), each recording at or nearly at noon, would, when the results were assembled, keep up a continuous history of the solar disk. With regard to proper preparation of paper, or the use of collodion, the photographic art is now so much advanced, that no difficulty can arise in fixing upon fitting preparations, or the manipulations necessary for multiplying them. But it would be very requisite that many impressions of each day's work should be taken and distributed, and an interchange kept up among observers.

5. *Preservation of Insects.*—We have received from Mr. Wm. S. Van Duzee of Buffalo, N. Y., a specimen of a method which he has devised for the arrangement and preservation of a cabinet of insects. He proposes to take glass-stoppered jars with a large mouth, attach to the under side of the stopper a rectangular strip of cork or soft board (whitened with paper or otherwise) as broad as the stopper and as long as the jar will hold; then arrange the insects on one side of this upright piece. The insects thus arranged would show finely in a collection,—could be always on shelves for exhibition—would be perfectly secure from insects, even without camphor, though it may still be used,—and the jars could be easily opened and insects taken out for special examination. Moreover, he suggests that the insects may be numbered, and on the opposite side of the board the names of the species may be given.

6. *Geological Survey of Great Britain*, (Athen., No. 1437.)—Sir R. I. Murchison has received the appointment of Director of the Geological Survey,—the post left vacant by the death of Sir Henry De la Beche,—and the salary of which is returned in the Estimates as £800.

7. *American Association for the Advancement of Science.*—The next annual session of the American Association will be held in Providence, Rhode Island, in August next, commencing with the third Wednesday of the month. Dr. John Torrey is the President for the ensuing year.

8. *Anesthesis.*—The Imperial Decoration of the "Majidieh" of the fifth class, a Decoration named after the Sultan, the Founder, has been conferred by the Sultan of Turkey on Dr. C. T. Jackson of Boston, as an expression of the Sultan's "estimation of his discoveries, and a mark of his Imperial favor."

9. Obituary.—*Sir Henry Thomas de la Beche*, (Ath., April 21, 1854.)—Sir Henry de la Beche was an example of that rare combination, a man of science and a man of the world. He succeeded in obtaining the end at which he aimed; and he has left in the Geological Survey and the Museum of Practical Geology enduring monuments. A notice of the labors of such a man,—who was thoroughly practical before the commencement of this practical age, and who aimed at educating the people in science long before the Great Exhibition made scientific education a fashion,—cannot but be of interest.

Henry Thomas De la Beche was the eldest son of Col. De la Beche: his family being descendants from the Barons De la Beche, who were settled at Aldworth, Berks, in the time of Edward the Second. He was born in London in 1796; but his youth was passed amidst the lovely valleys of Devonshire: his first education having been received at the School of Ottery Saint Mary. There is little doubt that the geological tendencies which were subsequently developed were due to the contemplation of nature in this locality and in the scenes around Charmouth and Lyme Regis—rich in organic remains,—which places were for some time the residences of his parents.

In 1810, Mr. De la Beche entered the Royal Military College, then at Great Marlow, but afterwards removed to Sandhurst; on leaving which he entered the army: but in a little time he resigned the profession of arms for the pursuits of science. For a man of wealth and fashion to devote himself to any study was in those days a phenomenon; and the adoption of a science then in its infancy and struggling into life, through the prejudices of the ignorant and the timid, was not a little remarkable.

Mr. De la Beche, however, gave himself up to the study of Geology and made it the business of his life. In 1817 he became a member of the Geological Society, then in the tenth year of its existence. In 1818 he married the daughter of Capt. Charles White, of Lough Brickland, County Down, Ireland, who died in 1844, leaving one daughter.

The year 1819 was spent by Mr. De la Beche in an examination of the geological formations of Switzerland and Italy, and his zealous prosecution of similar inquiries led to his being elected in that year a Fellow of the Royal Society. In 1820 a paper by Mr. De la Beche, "On the Temperature and Depth of the Lake of Geneva," the result of a most careful examination, was published in the Edinburgh Philosophical Journal. In his geological investigations of the British rocks the Rev. Wm. Conybeare, now the Dean of Llandaff, was, to some extent, connected with Mr. De la Beche; and his first communication to the Geological Society was the joint prodution of these two geologists,—announcing the discovery of a new fossil animal of the Saurian family, in the lias limestones of Bristol, which they named, as being distinctive of its species, the Plesiosaurus. From this time the name of De la Beche became closely connected with the science of the day. Many

valuable papers were communicated to the Geological Society, including an elaborate account of the Geology of Switzerland; the Fossil Plants found at the Col de Balme, near Chamouny; a communication on the Geology of the Coast of France; and several papers on the Geology of various districts in the British Isles,—especially of Southern Pembrokeshire, of Lyme Regis, Dorsetshire, and of Beer in Devonshire.

Mr. De la Beche possessed extensive estates in Jamaica. He now visited his property,—Halse Town, in the neighborhood of Spanish Town, and on his return, in 1825, he communicated to the Geological Society his remarks on the geology of that West Indian Island, of which nothing had been previously known.

Between 1827 and 1830, Mr. De la Beche, published numerous important geological papers in the Transactions of the Society, the Philosophical Magazine, and the Annals of Philosophy, and also a tabular proportional view of the superior, supermedial, and the medial rocks. In 1830 his first book, "Geological Notes," appeared; and in the same year, "Sections and Views of Geological Phenomena." Great skill in the use of the pencil enabled the author to furnish the whole of the drawings for these works, and to them all subsequent illustrators have been indebted. "The Geological Manual" was published in 1831, and was speedily translated into French and German,—becoming a text-book for geologists throughout Europe, and passing through several editions. In 1832 Mr. De la Beche proposed to the government to supply the data for coloring geologically the maps, then in progress of publication, of the Ordnance Trigonometrical Survey. This offer was accepted, and at the Land's End, in Cornwall, was commenced the great work of this eminent geologist's life. Mr. De la Beche, who bore himself the greater part of the expense of the Geological Survey of Cornwall, devoted several years to a careful investigation of all the conditions, lithological and mineralogical, of Western England; and he published a series of maps of Cornwall, Devonshire, and Somerset, which exhibited a correctness and detail such as had never before been attained. This Survey was fairly established under the Ordnance. "It was,"—says Sir Henry De la Beche, in his Inaugural Discourse, delivered at the opening of the School of Mines, on the 6th of November, 1851,—"It was while (in 1835) conducting the Geological Survey then in progress, under the Ordnance in Cornwall, that being forcibly impressed that this Survey presented an opportunity not likely to recur, of illustrating the useful applications of geology, I ventured to suggest to Mr. Spring Rice (now Lord Monteagle), then Chancellor of the Exchequer, that a collection should be formed, and placed under the charge of the Office of Works, containing specimens of the various mineral substances used for roads, in constructing public works or buildings, employed for useful purposes, or from which useful metals were extracted, and that it should be arranged with every reference to instruction; as by the adoption of this course, a large amount of information, which was scattered, might be condensed, and those interested be enabled to judge how far our known mineral wealth might be rendered available for any undertaking they are required to direct, or may be anxious to promote, for the good or ornament of their country."

Being supported in this recommendation, the nucleus of the Museum of Practical Geology was formed in an apartment in Craig's Court. This collection in a short time filled one house; and even when the Earl Marshall's Office adjoining was added to it, the Museum, by the exertions of its founder, was soon found to outgrow these buildings. A Laboratory was added to the Museum, and placed under the care of the late Richard Philips. The business of the Geological Survey was greatly extended, and the Palæontological department was superintended by the late Edward Forbes. The Mining Record Office was also, at the recommendation of the British Association, united to the Museum. In 1839, the sanction of the Treasury was obtained for Lectures on Geology, and its associated sciences, in their application to the useful purposes of life. Owing to the deficiency of room, it was not possible to commence these lectures until 1851; when the building in Jermyn Street received the valuable collections of the Museum and furnished the theater in which Sir Henry de la Beche delivered the Inaugural Address from which we have quoted.

In 1848, the honor of knighthood was bestowed on the Director of the Geological Survey; and in addition to this honor, in 1853 Sir Henry de la Beche was elected, by the suffrages of forty-seven members, Corresponding Member of the Academy of Sciences of Paris. The Order of the Dannebrogg was bestowed on him by the King of Denmark; and he received the Order of Leopold from the King of the Belgians.

Beyond the works and papers which we have enumerated, Sir Henry de la Beche published a voluminous report on the 'Geological Survey of Cornwall, Devonshire, and West Somerset,' 'Researches in Theoretical Geology,' and 'How to Observe.' In the various journals will be found forty papers and memoirs; and in 1851 Sir Henry de la Beche completed his last work, 'The Geological Observer,' founded upon his former work 'How to Observe.' In all these productions will be discerned a minuteness of detail and an excellence of illustration which mark the rare union of a skillful scientific observer and a finished illustrative draughtsman.

Although paralysis was seen by his anxious friends to be slowly but surely spreading its fatal influences over his once energetic frame, Sir Henry de la Beche would not allow himself repose. The labors of the Geological Survey and the business of the Museum engaged his attention daily,—and even two days before his death he spent several hours in the Museum directing the business of that establishment with his usual acuteness, although then powerless to move himself.

Sir Henry de la Beche raised for himself a splendid memorial of his talents and his zeal, and he created for the public an establishment which cannot but prove eminently useful, if it be carried onward in the spirit and with that well-defined idea—which has been the creative power and the sustaining influence—under which the Museum of Practical Geology and the School of Mines were formed and have been supported.

G. B. Greenough.—Seldom does it fall to our share of melancholy duty to record two such losses in one week as De la Beche and Greenough. The two men had something in common besides devotion to a common

pursuit and the coincidence of their deaths. Both were men of fortune. Both were designed for a worldly career. Both abandoned more ambitious schemes in favor of science. Both achieved solid reputation. Both were hard workers as well as clear thinkers; and they enjoyed in common that faculty for organization which is rarer in Englishmen than the faculty of observation. The Geological Society is the monument of Mr. Greenough, as the Museum of Practical Geology is that of Sir Henry De la Beche.

Mr. Greenough was born in 1778, and consequently was seventy-seven when he died, was eduated at Cambridge and Göttingen, and served in Parliament for the famous, or infamous, borough of Gatton. But he made no great figure in the House of Commons. His genius was a genius for map-making, not for speeches and legislation; and the records of his zeal which remain to tell posterity of his useful labors are 'The Geological map of England and Wales,' the map of 'Hindüstan,' and the 'General Sketches of the Physical Features of British India.' He was on a journey to the East, in hopes of collecting materials for new maps, when he died at Naples: and it is understood that he has left behind a vast accumulation of materials, some of which will doubtless be available for the press.

Mr. Greenough had a great reputation among men of science, without being very widely known to the British public. He was a worker, not a writer. More than thirty years ago he published his one volume, 'A Critical Examination of the First Principles of Geology.' Addresses and discourses to scientific societies followed from time to time; but not with that persistence of assault by which literary fame can alone be carried. Yet was Mr. Greenough considered by English geologists as the leader of their band, and he was one of the founders and was the first President of the London Geological Society.—*Athen.*, April 21.

10. *A Treatise on the Differential and Integral Calculus, and on the Calculus of Variations;* by Edwd. H. Courtenay, LL.D.—This work, though not published till after the death of its accomplished author, has been written with great care. No work on the Transcendental Analysis has issued from the American press, since the republication of Young's Treatises, so full and extended as this. A noticeable peculiarity of it, among American and English works on Analysis, is the absence of all examples for practice. Every principle is copiously illustrated by examples wrought out at length by the author; but beyond this no examples swell the pages of the work. Students will be much obliged to the author for the simple manner which he exhibits (p. 55,) of finding the successive differential coefficients in the applications of Maclaurin's theorem, without the labor usually employed.

The chapters on Maxima and Minima, that on the Integration of Trigonometric and Circular functions, and the one on Singular Solutions, will attract the attention of every reader by their fullness and perspicuity. A. R.

11. *Practical Meteorology;* by John Drew, Ph.D., F.R.A.S. 290 pp., 12mo. London, 1855. John Van Voorst.—This small volume was prepared in order to give to those who may be interested in practical meteorology, such information upon the general principles of meteorology in its different departments, and on the nature, choice and use

of instruments, as would be a sufficient guide to the learner as well as a help to the more experienced. Its object therefore is to teach "how to observe" meteorological phenomena. Besides practical and simple theoretical observations, there are good figures of meteorological instruments on copper-plates through the volume, and also a number of tables, such as, for the Tension of Aqueous Vapor; Comparison of Thermometric Scales; Degree of Humidity from Readings of the Dry and Wet Bulb Thermometer; Measuring Heights with the Barometer; Weight of a cubic foot of air saturated with different degrees of moisture, etc. Dr. Drew is a member of the Council of the British Meteorological Society.

12. *Report on the Agriculture and Geology of Mississippi, embracing a sketch of the Social and Natural History of the State;* by B. L. C. WAILES, Geologist of Mississippi. 1854.—This handsome octavo volume of 371 pages, with a map and other illustrations, is published by order of the Legislature of the State. The heads are: 1, Historical Outline; 2, Land Titles; 3, Agriculture; 4, Geology; 5, Fauna; 6, Flora, with an Appendix.

The means provided for the publication being inadequate, the author made a partial abridgment and was constrained to omit most of the plates.

Although the work was therefore for some other reasons executed under disadvantages, it does honor to the skill and industry of Col. Wailes who has been long known as an active cultivator and devotee of science. The historical sketch occupies 125 pages, the agricultural 79, the geology 90, and other topics of natural history 60 pages more. If the civil history occupies one-third of the volume it is information of interest to the State. We learn from it that the possession of that fine country was not obtained without severe vicissitudes, and distressing struggles continued through almost two centuries in which the French and Spanish and English nations and powerful tribes of aborigines, and finally the Anglo-Americans were active agents. The portion on agriculture is instructive, especially in relation to local interests. That on geology contains interesting facts in relation to palæontology, drift, fountains, lignite, &c.

We trust that the legislature of Mississippi having begun so well, will not hesitate to encourage their able and active geologist in extending and perfecting his researches which have thus far redounded to the honor and substantial advantage of the State in its most important material and social interests. B. S. Sn.

13. *Natural History of the United States.*—The following Prospectus has been issued by Prof. AGASSIZ with reference to a great American work which he has commenced. All lovers of science through the land will rejoice in this prospect of the publication of Prof. Agassiz's researches.—EDS.

Contributions to the Natural History of the United States, in ten vols. quarto; by LOUIS AGASSIZ. To be published by Messrs. Little, Brown and Co. of Boston, Mass.—For more than eight years, I have now been in this country, devoting my attention chiefly to the study of those classes of the Animal Kingdom which American naturalists have, thus far, not fully investigated. The amount of materials I have

already brought together is so great, that the time seems to me to have come when I should proceed with the publication of the more important results of these investigations. Desirous of contributing my share to the rapid progress natural sciences are making at present in this part of the world, I wish to present my work to my fellow-laborers in this field in the form most easily accessible to them. It has therefore appeared to me desirable to bring it out in a series of independent volumes. This plan will, moreover, leave me entirely free to present my contributions to science with such minute details, and to such an extent, as I shall deem necessary to the fullest illustration of my subject.

Without entering into a detailed account of the contents of this work, it may be sufficient here to state, that it will contain the results of my embryological investigations, embracing about sixty monographs, from all the classes of animals, especially selected among those best known as characteristic of this continent; also descriptions of a great number of new genera and species, accompanied with accurate figures, and such anatomical details as may contribute to illustrate their natural affinities and their internal structure.

I shall not extend my publications to classes already illustrated by others, but limit myself to offering such additions to the Natural History of the States I have visited as may constitute real contributions to the advancement of our knowledge.

From a careful estimate of the materials I have now on hand, I am satisfied I shall be able to include the most valuable part of my investigations in ten quarto volumes; each volume containing about 300 pages, with at least twenty plates. I therefore now open a subscription for such a work, in ten volumes, quarto, in cloth binding, at the price of twelve dollars each volume, payable on delivery. Each volume shall be complete in itself, containing one or several independent monographs; so that if any unforeseen difficulties should interrupt the publication of the whole, the parts already published shall not remain imperfect. As far as possible, I shall always select first such of my papers as contain the largest amount of new matter, or may contribute most directly to the advancement of science. Having devoted the greatest part of my time to the investigation of the embryonic growth of our animals, I shall make a beginning with the embryology of our turtles, several of which I have traced through all their changes. I trust this monograph will afford our medical students a fair opportunity of making themselves familiar with the modern results of one branch of physiology, which has the most direct bearing upon their science, and for which the different species of the family of turtles found in every part of the United States will afford them a better opportunity even than the artificial breeding of hens' eggs. Moreover, the extent of my embryological researches, covering as they do, all the classes of the animal kingdom, will furnish, I trust, a new foundation for a better appreciation of the true affinities, and a more natural classification, of animals. I foresee the possibility, upon this basis, of determining, with considerable precision, the relative rank of all the orders of every class of animals, and of furnishing a more reliable standard of comparison between the extinct types of past geological ages and the animals now living upon earth.

I shall have frequent opportunities of acknowledging the many favors I have received from naturalists of all parts of the country, from the Atlantic to the Pacific coast, and from the shores of our great Lakes to those of the Gulf of Mexico; and also of mentioning the many specimens which have been furnished to me from every part of the Union, and of which I shall publish descriptions.

It is a matter of course, that a work like this, illustrated by a large number of plates, cannot be published without a liberal and extensive patronage. As it has been prepared solely with the view of throwing additional light upon the wonderful diversity of the animal creation of this continent, its structure, and its general relation to that of the other parts of the world, without the slightest hope of compensation for myself, I trust I may meet with the approbation of those conversant with the importance of the subject, and receive sufficient encouragement from the enlightened part of the community to enable me to bring to a successful close an undertaking upon which I enter now, and in this form, for no other purpose than to contribute my share towards increasing the love of nature among us.

As the printing of this work cannot begin until a sufficient guarantee is secured for the publication of the whole, I take the liberty of making an appeal to the lovers of science to send to the publishers their own subscriptions, and such others as they may procure, as soon as convenient, and, if possible, before the first of August next, that I may be able to proceed at once with a work, which, relating to animals peculiar to America, I wish to make, in every respect, an American contribution to science, fostered and supported by the patronage of the community at large.

To render this work more generally accessible, it is intended to publish at the rate of about one volume a year. Such an arrangement will bring the whole within the reach of every student of Natural History, and of every friend of the progress of science in the country. The periods of publication, however, cannot be more definitely fixed, because the required uniformity of execution of the plates, to which particular attention will be paid, will demand that they be all entrusted to the same artist, who has drawn on stone most of the plates of my former works.

14. *Report on Crustacea;* by JAMES D. DANA.—The Atlas of the Report on the Crustacea collected in the Exploring Expedition around the world under Capt. Wilkes, U. S. N., has just been issued. It contains 96 plates in folio, containing figures and details relating to nearly 700 species, from drawings by the author. The figures are partly colored: as an introductory note states, a large part of the author's original drawings were unfortunately destroyed by a fire in Philadelphia, after the engravings were completed but before any copies had been colored, and on this account the number of colored figures in the Atlas is comparatively small. The engravings are well executed on copper. The Text of the Report, which was issued near three years since consists of two volumes quarto, extending in all to 1620 pages.

15. *Recherches sur les Crinoïdes du Terrain Carbonifère de la Belgique;* by L. DE KONINCK and H. LE HON, with a notice on the genus Woodocrinus, by L. DE KONINCK, D.M., 218 pp., 4to, with 8 litho-

graphic plates. Bruxelles, 1854.—The labors of Prof. de Koninck have largely contributed to our knowledge of the Fauna of the Carboniferous Period. These researches on the Crinoids are carried forward with the same thoroughness and precision as his former labors. The work opens with a catalogue of all the works or memoirs which have been published upon Crinoids, commencing with Agricola, in the 16th century. It continues with historical and critical observations, an account of their structure and the nomenclature of the parts, and then enters upon the descriptions of species.

16. *Ueber die Gattungen der Seeigellarven.*—Seventh memoir on the Metamorphosis of Echinoderms; by JOH. MULLER. 56 pp., 4to, with 9 copper plates. Berlin, 1855.—From the Transactions of the Royal Academy of Sciences at Berlin.—This is a continuation of the author's elaborate and beautiful researches on the Echinoderms. The plates are admirable.

C. LYELL: Report on the New York Industrial Exhibition, 50 pp. fol. *London*, 1854.—This Report gives a general review of the geology of this country, and of its economical products, and was written in the main by Mr. James Hall, of Albany.

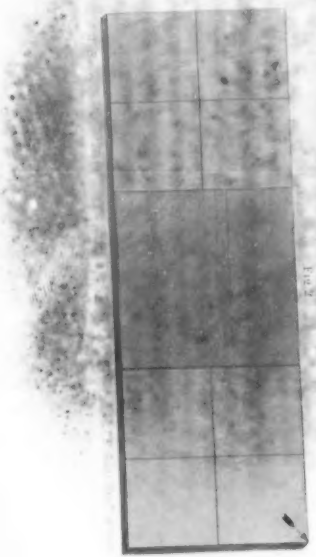
W. HAIDINGER: Ueber den Felsöbanyt.—Der Partschin von Olahpian.—Note über gewundene Bergkrystalle.—Beitrag zur Erklärung Farben der Polarisationsbüschel.—Ueber den Pleochroismus und die Krystallstructur des Amethystes.—Mittheilung aus einem Schreiben des Herrn Prof. Stokes über das optiche Schachbrettmuster.—Dauer des Eindruckes der Polarisation-büschel auf die Netzhaut.—Die Richtung der Schwingungen des Lichtäthers im polarisirten Lichte.—Einige neuere Ansichten über die Natur der Polarisationbüschel.—Pleochroismus einiger Augite und Amphibole.—Form und Farbe des Weltzienits.—Pleochroismus an einigen zweiaxigen Krystallen in neuerer Zeit beobachtet.—The above important papers are published in the Sitzungsberichte of the Royal Academy at Vienna, for 1854.

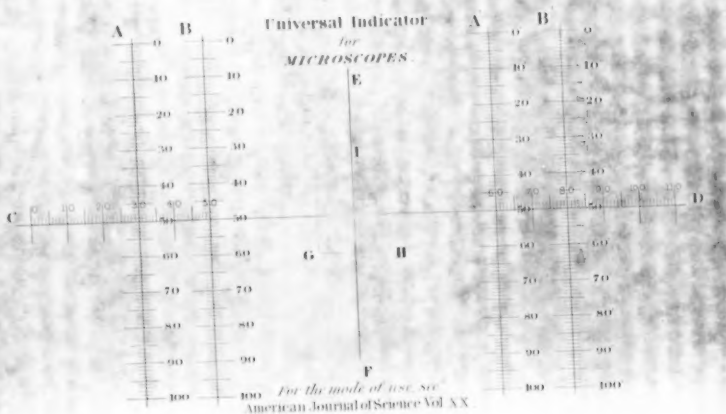
GRAILICH and PEKÁREK: Das Sklerometer.—An instrument for measuring the hardness of crystals, 30 pp., with a plate. Vienna Sitzungsb., 1854.

PROCEEDINGS OF THE ACADEMY OF NATURAL SCIENCES, PHILADELPHIA, vol. vii. —No. 8.—p. 287, note on some animals of the Falkland Islands; *F. Bernsée.*—p. 290, Analytical table of the species of Hydroporus found in the U. States, with descriptions of new Species; *J. L. LeConte.*—p. 299, Synopsis of the Lathridiides of the U. States and northern contiguous Territories; *J. L. LeConte.*—p. 306, Notes on Birds of New Mexico; *T. C. Henry.*—p. 317, Analysis of Meteoric Iron of Tucson; *F. A. Genth.*—p. 318, On the Viviparous Fishes of the Pacific Coast; *G. Girard.*—p. 328, On Tertiary strata of Infusoria and Polythalamia, near Monterey, California: *W. P. Blake.*—p. 331, Characteristics of some new species of N. American Mammalia: *S. F. Baird.*

PROC. BOST. SOC. NAT. SCI., vol. v.—p. 161, On the Cochituate Water; *C. T. Jackson*: ibid.; *Dr. Bacon* and *A. A. Hayes.*—p. 165, Fossilized Eggs from the Guano Islands of Peru; *A. A. Hayes.*—p. 169, On the Cochituate Water; *A. A. Hayes*: ibid.; *C. T. Jackson.*—p. 180, On the Flight of the American Passenger Pigeon, at Madison, Indiana; *P. H. Greenleaf.*—p. 182, On Impressions of footprints, etc, in the red shale under the Coal measures of Pennsylvania; *H. D. Rogers.*—p. 188, On Fossil Rain-drops.—p. 189, Allanite at Manchester, N. H.; *C. T. Jackson.*—p. 189, On lignite of Virginia, and N. Carolina New Red Sandstone; *W. B. Rogers.*—p. 191, On the chemical characters of the water which enters the sea below Boston; *A. A. Hayes.*

PROC. NATIONAL INSTITUTE, WASHINGTON, Vol. I, No. 1.—p. 2, Life in its Physical aspects; *C. Girard.*—p. 23, Processes and results of Arithmetical Calculations of the Elements of the orbit of the Moon and of the principal Lunar Equations; *S. E. Coues.*—p. 42, Determination of altitudes with the barometer; *L. Blodget.*





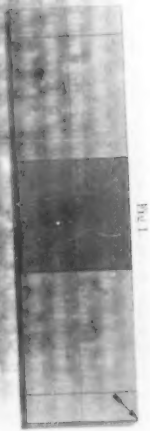


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Fig 1

Fig 2

PLATE I